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MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY.

EDITED BY

PROF. G. E. MOORE.

WITH THE CO-OPERATION OF F. C. BARTLETT, M.A., AND C. D. BROAD, LITT.D.

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I.—HUME WITHOUT SCEPTICISM (I).

BY R. E. HOBART.

THERE are two connected doctrines set forth by Hume that are commonly deemed of sceptical import and were so regarded by Hume himself. But there is ground for holding that, taken at their greatest depth, they are analyses of fact without sceptical consequence. These are his doctrines respecting the nature of cause and of induction. There are certain other conceptions of his to which the same applies, but let us confine ourselves here to these two.

In regard to each the questions that concern us are : What is the thought in essence ? Is it true ? and Is it sceptical ? The present article will endeavour to meet the first two of these, a second article, based in part upon it, the last. In stating the essential doctrine perhaps a certain liberty may be permitted in the terms employed, the liberty to draw attention by their means to a slightly different aspect of his principle from that which he put into words, but an aspect inseparable from it. And we shall assume for present purposes, as does Hume in this portion of his work, the existence in some true sense of a physical world.

With regard to cause and effect, Hume pried apart certain ideas that were really distinct but had grown together. A proposition may imply another proposition, but a thing cannot imply another thing. That is the whole discovery ; there is nothing more. We reach it by examining what is meant by a proposition and what is meant by a thing ; and what they re-

spectively are as presented for examination in experience. Implication is a relation that arises from what we call the "meaning" of terms and the "reference" of concepts; it is not a character of things. Hume pried apart the idea of a thing from that of a proposition—and, in doing so, the idea of one thing from that of another. If we could see and know the nature of any concrete fact through and through (whether it be object or event or datum of sense or all of these that we deem concrete), we could never find from it that some other fact existed or would exist. Nor from a hundred or a thousand facts, nor from the whole universe of facts minus the one to be inferred,—so long as we looked at their intrinsic nature alone. One fact is itself merely, and another fact is *itself*. Even if we pronounce them interdependent, or parts or aspects of one reality, the principle holds good; each fragment tells us nothing of the rest. There is dependence in fact but not implication by nature. The relations of a thing in space-time may be in one or another respect required by us in order to distinguish and identify the thing, but they are not discoverable in its intrinsic constitution. Further, could we frame a proposition exhaustively describing and asserting a particular concrete fact, so long as we included only what was necessary to assert this fact, *that* proposition would not, for the same reason, imply the assertion of any other concrete fact. By no deductive process can we infer one particular from another, except with the aid of a previously known law that conjoins them. If we rest our conclusion on such a law, then there is no longer any question of implication by a concrete fact, and there remains only the other problem, how we come to know the law. Hume says that we gather it from experience. Prior to any deductive process is a process of natural learning. We come to be aware that one thing is a sign of another. Signs are something that we must have learned to read; and that we never can from examining the sign itself alone. We learn it only from experience, which shows us data occurring in certain combinations and thus breeds expectancy in the mind, which, when one appears, foresees another. How far such a mode of inference satisfies the intellect is a topic of the second article.

Thus it is not written in the character of any concrete fact now existing that it will exist at any other moment or continue to exist through a succession of moments. The hardest of steel bars does not intrinsically promise to remain in existence nor that its constituent material will do so; it might at any instant cease to be—go out like a candle-flame—so far as strictly

internal evidence can prove. It is experience that declares a steel bar likely to go straight on existing.

Neither is it written in the character of any event that a certain other event will surely follow. That it will follow may be true, and, if the name "cause" is justly bestowed, it is true; but it is not a fact resident or traceable in the first event. Within that event the coming of the other could not be discovered because it has not happened yet—because they are two distinct events. "The pre-existence of the effect in the cause" is a contradiction in terms. To say that the cause implies the effect (except in the well-warranted popular sense of being a sign of its coming) is merely to fall into a confusion of ideas. The statement, closely looked at, is unmeaning. The second event is not mentioned in the first nor is it part of something mentioned. For nothing is mentioned. That the second will follow is a proposition and it may be a truth, but the first event does not utter propositions, it whispers nothing to us, it merely presents itself; nor, apparently, (if a physical event) does it think truths, or dream on things to come, or will them to come; and, if it did (a point of great importance), it could not prove out of itself that its utterance or its thought was true, or its volition destined to be obeyed.

Hume's deepest doctrine is not that we cannot discover the secret in the constitution of the one fact by which it implies the coming of the other, nor yet that we cannot know whether there is such a secret or not; but that there is not and could not be any such secret.

In examining the import of the doctrine we are confronted at the threshold with what I cannot but think are two vital misconceptions. First, it has been imagined that his reasoning would take the living force out of nature, the pushing, driving, compelling force that the natural man imputes to physical things; comparable in some sort to what we feel in ourselves when we exert our strength upon matter; that the doctrine would leave only spectral forms, "geometrical ghosts," instead of a matter that contains its operating principle within it. Second, it has been taken for granted that the doctrine denies the necessity with which mankind have supposed that effects followed upon their causes. These two topics are next in importance to the foundation-thought itself. To them we shall proceed as soon as we have faced objections to that foundation-thought.

According to that thought the conception of cause is concerned solely with time—with the temporal conjunction of existences.

The conception involves "before and after," "whenever" and the taking of the antecedent as a *kind* which may recur.¹ By the cause of an event we mean, and can only mean, its sufficient occasion; those circumstances upon the conjunction of which it will necessarily follow. (I say "occasion" instead of "condition," as the latter would seem to mean that the effect could not at any time have another cause, which, whether true or not, is not embraced in the meaning of the word.) It is not required that this conjunction should ever in fact recur; its being a cause means only that the effect would follow if it did recur. Moreover, the causal relation is commonly quantitative and must be expressed as a relation between any quantity of one species of fact and a proportional quantity (whatever the proportion) of another. I speak for simplicity's sake as if the cause must always precede, but of course the meaning of the term allows it to be applied to circumstances upon which another fact depends for its continued existence, that is, upon whose coming to an end it would cease to exist. In this respect also I am concerned with the meaning of the term and not with the facts of nature.

Professor Whitehead accuses Hume, however, of "an extraordinary naive assumption of time as pure succession. . . . Time is known to us as the succession of our acts of experience and thence derivatively as the succession of events objectively perceived in those acts. But this succession is not pure succession: it is the derivation of state from state, with the later state exhibiting conformity to the antecedent. Time in the concrete is the conformation of state to state, the later to the earlier; and the pure succession is an abstraction from the irreversible relationship of settled past to derivative present. The notion of pure succession is analogous to the notion of colour. There is no mere colour, but always some particular colour such as red or blue: analogously there is no pure succession, but always some particular relational ground in respect to which the terms succeed each other. The integers succeed each other in one way, and events succeed each other in another way; and, when we abstract from these ways of succession, we find that pure succession is an abstraction of the second order, a generic abstraction omitting the temporal character of time and the numerical relation of integers."²

A conception of time that omitted its temporal character would certainly be regrettable. What Dr. Whitehead here principally

¹ Hume includes provisionally contiguity in space as an essential, but subsequently waives it.

² *Symbolism*, 40-41.

means to tell us seems to be that there is a fact of derivation which experience reveals and Hume misses. But why should this be put in the form of a charge that he misconceives time? The notion of succession is an abstract notion, to be sure; we abstract from the concrete facts that succeed one another in order to fix attention on their successive character. Derivation is another abstract idea, which involves mere succession and, as we have seen, something more; it cannot be defined without mentioning succession. On what ground should we reverse this and say that succession involves derivation? No ground is offered. Surely it does not. We can think of casual happenings, one succeeding the other, without thinking of the one as derived from the other and even without taking the trouble to think of their derivation at all. Hence we can employ "succession" as one idea and "derivation" as another and more complex one. It would be unfortunate for our logic and discrimination if we could not. When Dr. Whitehead writes "later," "earlier," "antecedent," "past," "present," "succeed each other," he is apparently thinking incidentally just of succession. "This succession is not pure succession; it is the derivation of state from state." This seems to mean that succession as found in fact is always the succession of states the later of which are derived from the earlier. We need not debate this proposition; if true, it would not, in the respect in question, be surprising. Concrete specimens of a class have other characters besides the class-character; we must be careful not to identify these with the class-character. What is successive may be also derivative, and yet succession does not imply derivation. Just as there is no mere colour but always some particular colour, so there is no mere succession but always some particular succession; but to say this is not to say anything about a "relational ground." (Though Hume, of course, would not deny a relational ground, but only analyse the idea. Incidentally, "a ground" seems sufficient, since "relational" does not appear to convey anything not implied in the noun.) The past is "settled," yes; it cannot be held that something both was and may not have been. The present is "derivative," yes; the objects that exist now have already existed; present events have had previous events for their causes. But here "the past" and "the present" do not mean merely portions of time but concrete facts in time, that which is past and that which is present. That what is past is "settled" belongs to the nature of time itself, but that what is present is derivative has in no wise been shown to be involved in the nature of time. To upset old distinctions is well if it is neces-

sary for accuracy, but not unless it is necessary. In the passage quoted we are given to understand that Hume confuses the abstract and the concrete. But it is not he who confuses them.

Dr. Whitehead conceives that "every actual thing is something by reason of its activity; whereby its nature consists in its relevance to other things, and its individuality consists in its synthesis of other things so far as they are relevant to it."¹ "... every item of the universe, including all the other actual entities, are (*sic*) constituents in the constitution of any one actual entity."² He fastens on the case of perception; have we not here at least something that by its own nature tells of other facts? Suppose that every actual entity *perceives* or *feels* other things; does not the universe exultantly escape from Hume's generalisation? Now so far as perception is presentative we have through it no datum telling us of fact other than that datum. So far as it is representative we have through it no certainty that the represented object exists. Reference (the actual nature of which is not our topic here) does not carry implication with it, for the shadow of possible error hangs over it. In other words, in neither case have we before us a datum that implies the existence of something else. Of course, it could be held that, not in a datum gained through perception, but in the act of perception itself (conceived as presentative) we have to recognise something whose nature it is to imply something else. This would suppose not only that the act of perception can be distinguished, as another concrete existence, from the object of perception, but that an intellectual examination of the former alone, if adequate, would show that its particular object must exist. Who would maintain this latter? If we did maintain it we should have to add that this act of perception is not open to observation by an investigator who does not already know the particular object, and hence can furnish no aid to a system of induction that bases itself on the implications to be found in each actual thing.

"The aspect from A of some other region B"³ purports in perception, as Dr. Whitehead recognises, to be located at B, not at A at all. It might be described merely as perceptible by a subject whose body (in a very inclusive sense) is at A. If we locate the aspect in any sense at A, if we call it "a mode of a sense-object at A," if we say it is "present at A with the mode of location at B," we do so at the exigence of an attempt to com-

¹ *Symbolism*, 31.

² *Process and Reality*, 205.

³ *Science and the Modern World*, 99-100.

bine a presentative and a representative theory of perception. (I do not intimate that such an attempt is illegitimate—or the reverse; I am not treating of the problem of perception.) On a purely presentative theory B would not be at A in any sense or respect, though a state of things at A would be an indispensable condition of the presentation. On a purely representative theory the real B would not be at A. But when we say that an “objectification” of B exists or is present in some true sense at A, we are in effect combining features of the two theories. We are passing beyond the datum of perception in order to unite the fact of perception with the fact of physical impression. Perception from its own point of view contains nothing that *renders it intelligible* that the object should have different locations at once or that what is in different locations should be termed the same object. In so far as the given aspect is truly a fact present at A and therefore intellectually examinable as such, it cannot prove out of itself that the object at B exists. In so far as something is actually at A, it cannot insure the existence of something not at A. In a complete detail of the former fact will not be found the latter fact in its own actuality. Moreover, no perception can tell us purely of itself that the object or aspect given will be given to any other perception, is otherwise discoverable as a fact in nature, or exists when the perception does not. It is experience that tells us these things, so far as we know them.—It only remains to submit that in any case Dr. Whitehead has not proved that all “actual things” perceive or feel each other.

His general conception rests ultimately of course on familiar facts of mutual influence in nature. But Dr. Whitehead cannot express his own philosophic conclusion from these facts without the use of terms that deny his conclusion. He cannot tell us to which thing he is referring without distinguishing it from other things. He cannot tell us that location is not “simple,” that the thing has an “objectification” elsewhere, without distinguishing this from that, which is all that Hume’s analysis requires. Grant nature as Dr. Whitehead conceives it, so far as the concrete facts go, and it will still be true that if you catch any “actual thing” it will be merely itself, however interesting its constitution and its relations; and that the reverberations or reflections it is said to contain of other things in nature will be facts, in their numerical identity, not to be found in those other things or else not to be found in it. One thing requires others “for its explanation,” truly; that is what we mean by its being dependent upon them; but not for its distinct identity;

we could not think of it as dependent upon them without thinking of it as distinct from them. Hume does not assert "individual independence," as Dr. Whitehead alleges,¹ but merely individual distinctness, and so by implication does Dr. Whitehead himself. He ascribes to Hume "the clarity of genius." I cannot think that genius is always clear; I could wish that Dr. Whitehead were clearer. But what it is important to recognise is that the endeavour, characteristic of Hume, to distinguish ideas clearly is in itself fully consistent (though not in him especially conjoined) with knowing and feeling how interfused, how elusive, how mysteriously operative are the elements to which those ideas apply.

Dr. Whitehead tells us, further, that there are two modes of perception, one showing us objects separate from each other in "presentational immediacy," the other being a perception of the conforming of something which temporally comes to something which temporally goes; a perception of influence and derivation. This latter is the more primitive mode. "We conform to our bodily organs and to the vague world which lies beyond them."² "The present fact is luminously the outcome of its predecessors, one quarter of a second ago."³ We are "conscious of relevance." Hume "tacitly assumes that presentational immediacy is the only mode."⁴ "In the dark, the electric light is suddenly turned on and the man's eyes blink. . . . The philosophy of organism accepts the man's statement that the flash *made* him blink. . . . [Hume] first points out that in the mode of presentational immediacy there is no percept of the flash *making* the man blink. . . . Hume refuses to admit the man's protestation, that the compulsion to blink is just what he did feel. The refusal is based on the dogma that all percepts are in the mode of presentational immediacy."⁵

There could hardly be a profounder misunderstanding of Hume's thought. He is not denying causation or derivation, or what we mean by the word "made," he is analysing it. He does not say that it is an illusion and that another type of fact is the only real type; he says that the real production of one event by another, in the common human sense of these words, may be proved to consist in what he specifies. His analysis applies not to one mode of perception only, but to any and all modes. To have declared that the words "made him blink" have a meaning not to be found in the facts as he alleges them

¹ *Process and Reality*, 191, 192.

⁴ *Ibid.*, 61.

² *Symbolism*, 51.

³ *Ibid.*, 54.

⁵ *Process and Reality*, 245-7.

to be would have been to confess the bankruptcy of his undertaking, to stultify his argument. His "sceptical doubts" (not in these articles accepted) do not concern the idea of cause and effect, but the validity of induction in the light of their true nature. His account of their nature was for him of sceptical import, as mentioned at the outset, only in that it made us sceptical about induction. (Of course, without induction, logical or instinctive, we do not, according to him, reach a causal law or know that anything is actually a cause; what I mean is that the universal human *conception* of cause is not rejected by Hume as involving anything impossible or inadmissible.) He analyses in effect the idea of "conforming to" and the idea of an "outcome." To analyse is not to destroy, though owing to the unreadiness of our imagination to put the simpler ideas together in the manner required it may seem to be so; that is the principle which Dr. Whitehead's entire discussion of this topic insufficiently seizes. It is possible to hold that Hume's analysis fails, that some essential fact in causation is left out of it. But in that case it seems necessary first of all to confront it, to recognise what it would say on each aspect, and to point out the spot where it falls short. It does not seem sufficient to assume from the outset that causation is something not provided for in it. Hume says that the flash makes us blink in that, given the circumstances, the blinking does inescapably follow upon it; that to *make* a thing happen means to occasion it, or be the occasion of it, unconditionally—without chance of another sequel—and could not intelligibly mean anything else. The flash makes us blink, that is, when it occurs we blink forthwith as a matter of course and cannot help blinking. This is what our experience testifies and our protestations carry no other content. Dr. Whitehead could do nothing more felicitous than to fix attention upon experience. The flash calls the blinking into existence; that is, when it calls, the blinking comes into existence. In this there will remain some semblance of paradox until we have looked closely into the nature of force and of necessity; the point here is that it is Hume's teaching. The knowledge of acting and of being acted upon is primitive of course; a single sequence in experience has already created some tendency to expect; and the knowledge plays that vital part, close to our emotions, that Dr. Whitehead allots to it. "When we hate, it is a man that we hate and not a collection of sense-data—a causal, efficacious man." Manifestly. One cannot but imagine Hume's smile at the remark. His exposition is an appeal to what may be called our analytic imagination to perform the unaccustomed task of

realising what in detail we mean by a causal, efficacious object or a causal, efficacious man. The quoted remark is not opposed to his conception; a collection of sense-data does not include the usual sequels of those sense-data; nay, (since we are not here concerned with his theory of perception, of which his reasoning about causation is independent) an object or an entity, *as it is at the moment*, does not include or imply its usual mode of action; and it is evident that when we hate we are thinking of a man not merely as he strictly is at the moment but *plus* his modes of acting, which we have learnt from experience. As for a "direct perception" of causal action, such action is going on, and we might conceivably perceive all that is going on. The only thing that we cannot at the moment of the action perceive (though we may vividly apperceive it) is the fact that it is truly to be classed as causal; for that means that if the predecessor recurs it will have the same successor. This cannot be perceived now because it would be an event in the future. We could conceivably perceive all that is there, but hardly what is not there. As to "the man's protestation that the compulsion to blink was just what he did feel," surely what we do at the moment is to *recognise* familiar sequence,—the succeeding of the second event as a matter of course. On the first occurrence we were familiar with the idea of causing and here was something that had every indication of being a case of it—we recognised the sequence as causal—if we noticed the fact of blinking at all. Dr. Whitehead holds that the perception of causation is consummated in the moment, that its nature is completely given in the moment. Now when we perceive it directly in one instance, do we perceive—or can we infer from what we perceive—that if the antecedent came again in the same circumstances so also would the successor? If not, it cannot be causation that we perceive, for that is of its essence; an event does not *make* something else happen if the same event could be there in identical circumstances without making it happen; in that case there must in the first instance have been more or less of chance. But if the answer is yes (and this it appears to be) then there must be something present in the antecedent which implies the sequel and hence gives us to know that it would recur,—the flash implies the blinking,—and this conception it is on which Hume turned his fatal analysis. It is not fatal to the idea of cause, else it would be no analysis of it, but to a certain philosophical impression, surely somewhat hasty and nebulous, as to what that idea contains. Furthermore, if time interposes one obstacle to an immediate perception that the cause is to be classed as a cause,

that it has the whole causal character, space and material structure offer another. Though conceivably we might perceive anything that is there, yet actually our perception is not wide enough to embrace all the circumstances upon which an event depends. There are outlying physical prerequisites, positive and negative, beyond the reach of our sense-organs; in the case of blinking there is first of all the neural and muscular mechanism connected with the eye; what we do perceive would not by itself produce the familiar effect. The person who says that the flash made him blink is prepared to hear that nerves and muscles were required too; he sees in this nothing that contradicts his statement; the flash in the general circumstances made him blink, though he could not at first undertake to specify all the circumstances required. Even the tendency of pushing to move matter would never be known to us without the constant presence of conditions, partly unperceived, that permit the matter pushed to move sensibly. How, then, even granting implication on the part of the cause, could we have a direct perception of "causal efficacy"? The cause is efficacious (in other words is a cause) only in its entirety, and to a single perception it is not in its entirety revealed. We must have experience, that is, more perceptions than one, to acquaint us with the real dependence of events on occasions and with the occasions on which they really depend.

"Universality of truth arises from the universality of relativity, whereby every particular actual thing lays upon the universe the obligation of conforming to it."¹ The latter clause means in practical effect, does it not? that each actual thing is probably or presumably one condition of the behaviour and nature of the remainder of the universe. But obligation? Where do we find it? Nay, what in this case do we mean by it? "Thus in the analysis of particular fact universal truths are discoverable, those truths expressing this obligation." If Dr. Whitehead could show us this, not in general statement only, but by analysing a particular fact! If it is true, then a particular fact is a package in which universal synthetic propositions come bound up. I say synthetic, because universal truths about events in nature take the form "A is attended with B," or "Any quantity of A is attended with a quantity of B corresponding in a certain proportion"; here A and B are two recognisably distinct facts; we are not entitled to lay down the law unless A is a complete sum of conditions, taken carefully in its

¹ *Symbolism*, 45, 46.

entirety, and B is distinguishable from the whole of it. On the other hand, the universal propositions are not discovered "in the particular fact" unless they are analytic, discovered "in the analysis," contained in the nature of the fact, so that we cannot assert that fact without asserting these propositions. The universal truths must be analytic, but they are synthetic—which is but another mode of stating Hume's thought with which we began.

So much as to the direct defence of implication by the cause or the effect. There are those, however, who do not directly insist that the nature of the cause implies the effect, but that one genuine instance of the sequence implies that it will always recur. In other words, one case implies a law. Or, if they would not say "implies," at least renders it in some sort self-evident. (It would seem that this opinion must rest ultimately on belief in implication by the cause, but let us waive that.) Now implication is in any case a clear instance of what we mean by self-evidence. An explicative or analytic proposition is self-evident. And in its case we can see and point out just why. We can see and point out a necessary relation. If in the case of the single sequence a necessary relation can be seen, why can it not be pointed out? If it cannot be seen, what right have we to say that the certainty is self-evident? What more do we mean than that we firmly believe in it? It is urged: "the causal relation which connects *a* with *x* connects a cause of the *nature a* with an effect of the *nature x*. The connection is between them *as a* and *x*, and therefore must hold between any *a* and any *x*, if they really are *a* and *x* respectively; in other words, it must be uniform. The denial of this is just the denial of universals."¹ We cannot deny universals because we do find the same kind of fact in different things. (It would not alter the argument except in wording to substitute "the same fact in different things.") But to say that one concrete fact which embodies certain universals is followed by another concrete fact which embodies a partly different group of universals—this is to admit universals, but it is not to say that on another occasion the second group will of necessity be found following the first. It is not to say anything about that. Mr. Joseph, however, in the words quoted has begun by calling the relation causal. If we do that we have already said that the sequence will occur as between any *a* and any *x*, in other words, it will be uniform. What is to be proved has been assumed at the outset. But if at the outset we do not

¹ Joseph, *Introduction to Logic*, 409.

assume any law, we confront just the fact that *a* exemplifies a group of universals and *x* exemplifies a group of universals and that on this occasion *x* comes after *a*. They are two groups, not one; we still have to say, Show us some reason why they should always go together. The oddity is that the quoted argument relies, to prove this (if we set aside the verbal assumption that the case is in fact one of cause and effect) not at all on any relation between the universals themselves, but on the brute fact that they are found on one occasion together. If we say that the two concrete facts are together "*as a* and *x*," we mean either "*being a* and *x*," which proves nothing further, or "*because they are a* and *x*," which already assumes a causal law. In neither case have we proved that law from our one instance. We have not disproved that the knowledge of causal laws comes to us from experience only.

On somewhat comparable grounds Mr. J. M. Keynes in his *Treatise on Probability* is inclined also to hold a view from which it would seem to result that one instance of a sequence, completely known, is of itself evidence of a law. He suggests that "we judge directly" that the consideration of past and future "is in itself irrelevant," and that hence past instances may justify a generalisation extending over the future. "If direct judgments of irrelevance are ever permissible, there seems some ground for admitting one here."¹

But what is it that we are said directly to perceive? An irrelevance. Of what and to what? The irrelevance of position in time to the law that A will be followed by B? But we have not got such a law yet. We have only the fact that A has in the past been followed by B. We cannot say, the irrelevance of the fact that a certain A is still in the future to the fact that all A's hitherto have been followed by B's; that irrelevance is undeniable but does not help us. Our direct perception is apparently no aid till we reach a point where it is superfluous. Shall we say, the irrelevance of the coming A's futurity as an objection to our inference that this A will be followed as A's have in the past? Well, let us admit that its futurity is completely irrelevant as an objection to an otherwise valid inference about it. But what is the ground of the inference? The character of the past A's sequels. But the objection is that *this* is completely irrelevant to the future A's sequel. The past facts are just themselves, and no information about other fact, future or not, can be extorted from their intrinsic character. It is not that we have a rationally

¹ Keynes' *Treatise on Probability*, pp. 256, 258.

natural inference, arrested by just one teasing objection, which may happily prove irrelevant, as to futurity; we have no rationally natural inference at all. Irrelevance is the difficulty, not the deliverance; relevance only would be that.

Again, there are those who are quite willing to give up the claim that one particular can by its inner constitution imply another and perhaps also that one case of their sequence can of itself imply a law, but who maintain that when it comes to a proposition asserting the existence of a particular the case is different. For in judging that it exists or existed, we place it in the system of time and therewith in the system of nature, in the world of acting substances. They grant that to infer one concrete fact from another we must have a law, but hold that we cannot so much as perceive either of them without presupposing a law, the law of cause and effect. To the declaration that we have the idea of cause only through experience came the historic answer that we have experience only through the idea of cause; that this category must be present in every one of the perceptions that constitute experience.

In support of this imposing answer Kant confronts Hume with a simple and fundamental proposition. It would be audacious to say that Kant's argumentation is simply presented, but it appears when examined to rest, in all its phases, on one assertion, without which it would at once collapse. Hume had in effect maintained, as we saw, that the relation of cause and effect is simply a matter of "before and after" and "whenever," that what it consists in is simply relations of time. Kant's reply is that *we cannot perceive or conceive mere relations of time*. We cannot perceive or conceive two data that have between them merely the relation of priority and subsequence. We can take cognisance of succession only as change in a substance and with a cause. We cannot have merely events, we must have something like a machinery of events. Hence Hume's notion, that we can begin by experiencing succession and advance to the idea of something that is always succeeded by the same, turns the facts upside down. We have to begin with the idea of determinate, that is causal, succession if we are to begin at all.

Now this proposition, on which all depends, that we cannot perceive a mere "before and after," is unhappily not substantiated. Kant reminds us indeed that there cannot be duration without something that endures, or change without something that changes. But these are verbal necessities. These particular words have something approaching to the strict implication he ascribes to them (though in our thought the something that

endures or changes is by no means always a substance); they are names belonging to temporal ideas already highly developed. But change thus strictly interpreted is not the only form of succession. We cannot say "Without the permanent there is accordingly no relation of time." Can we not hear sounds as successive without proceeding to think of the substance or mechanism from which they issue? Has the study, in the last half-century, of presented time, of what may be called the passing as distinguished from the past, been wholly astray in finding that data appear as simply before and after? Kant at least gives us nothing that would indicate it. (Such appearance raises problems, but they are not the problems of this article.) He points out that time is marked and measured by change in physical things. Of course; our full-grown idea of time, with its points of reference and comparison, could not be what it is without a continuing world (though it does not at all follow that this or that particular form of matter or motion could not be conceived as ceasing or beginning to exist quite of itself). But what he needs to prove is that there cannot be any apprehension of a bare succession, and to that purpose he offers nothing that will bear scrutiny.

That Kant's well-known chief direct argument as to cause does depend on the proposition about succession may be readily shown. Let us examine it as it stands without this presupposition. He points out that we have to construct our notion of the surrounding world from snatches of experience (now the top of a house and now a lower part) that do not represent, in the order of their coming, the relations of time that we ascribe to the objects themselves. We construct objective relations of time, co-existences and sequences, which are different from the order of our perceiving. Now when the objective time-relation is a sequence, as in the case of a ship moving down-stream, we have to construct it as a definite order that cannot be taken the other way, as could the parts of a house. We distinguish the constructed sequence from the sequence of our own glimpses—as being what? What do we mean by calling it objective? Why do we distinguish it from the subjective sequence? We mean that it is an order which is to govern our expectation and thinking in relation to experience, a determinate order with which our mind may not tamper, an order that has its own rule, so to speak, which is to be used as our rule for constructing it; "a rule," he goes on, "that necessitates the connexion of its various parts in a certain way," and he interprets this as involving relations of cause and effect. This argument, if understood

as making none but ordinary assumptions, would depend wholly on a confusion between three senses in which we may speak of a "determinate" order. In the first sense we mean a definite order, some specific order, perhaps merely conceived and referred to, as in mathematics; in the second we mean a definite order that must in some sense be recognised as objective, embodied in certain objective events; in the third we mean an objective order that is caused—that has causes, in or out of itself, which make it the order that it is. The second involves the first, and the third the second, with additions. In connexion with each of these three senses we may employ with some meaning the idea of "a rule." With the first there is the "rule" that the mind may not transpose the order thought of if it still means to think of the same order! With the second there is the same rule, but with an added authority for it, so to speak; it is now imposed also independently of our will; if we disobey we not only miss our aim, but we incur the penalties of misconceiving the world about us; even if only "phenomenal," it is a stiff and obdurate world. With the third there enters an entirely new sort of rule; the first and second are there, but there is now a rule, not for our thinking only, but of the phenomena themselves, namely that one must follow upon a certain other, or upon facts co-existent with it, whenever it or they occur. There is a special reason for stumbling into confusion because in Kant's philosophy (one phase of it at least) the phenomena are only what we have to construct or think of; the distinction between a rule for the mind and a rule or law of the facts seems to disappear. But it does not really disappear. The rule in the first and second cases says that we must think of and according to a certain definite order, whatever it may be. The rule in the third case says that we must think of and according to an order which is to be invariably respected, a norm for all time. Now if Kant were making no special assumption here, then he must be regarded as gliding over by the ambiguous use of such words as "determinate" and "rule" and "necessary" from the idea of an order determinate in fact, that is, some particular order, to that of an order determined by causes. He must be regarded as confounding "we have to think of it in that way" with "we have to think of it as happening in that way always—on every occasion when the first of its members occurs"; a completely different matter. Otherwise there is not the least necessity made visible that a succession, to be that particular succession and none other, should be conceived as having each of its members fastened down in its place, so to speak, so that

it cannot slip over into another place, by causes. The definite order is the particular order we mean and attribute to objective phenomena, that is all; any other would be another order. If you say that any definite sequence of events whatsoever must be causal, you are assuming the law of causation in your argument instead of proving it. If you say that a sequence conceived as independent of our will is thereby conceived as causal you are surely confusing two distinct ideas. If you mean that *every time* the phenomena occur, we can perceive them only in the same order, and if you infer causal character from this, you are using just that argument from experience which Kant is condemning. An objective sequence may be found to be connected by relations of cause and effect, but a sequence can be conceived as objective without implying any such connexion. As for the fact that we have to construct an objective world, the relations of time we attribute to it are obtained apparently by imagining those that would appear if our perceptions were complete instead of fragmentary, which we do as best we can with the aid of experience.

Assume, however, as Kant really does, that we cannot perceive or conceive data in an order of simple priority and subsequence, and the argument is at least less flagrantly fallacious. We *cannot* then construct what is merely some specific order in time; the only order we can construct will be one which has something else in it besides succession to fix it as a definite order. Its members must have some other relation to mark the place of each in the series. This is what Kant apparently means, or includes in his meaning, in his much-repeated remark "We cannot perceive time in itself." I do not linger upon the argument as thus founded, for the presupposition is unproved and, in the light of our experience, fantastic. We have to conclude then that to place a thing in the system of time (so far as that is required in order to judge that it exists or existed) is not thereby to assume the causal system of nature. The external natural facts related to the thing are not involved in its bare existence any more than in its bare quality.

Turning back from critics of Hume's analysis to our study of its consequences, we come to the two misconceptions already mentioned, as to force and as to necessity. The present state of opinion on the subject will be accepted as my excuse for what would otherwise be more than a due explicitness.

The assumption that Hume banishes "force" or "enforcement" either misses the fact that what he has his eye upon is implication, or else it hastily takes for granted that such a force

would imply its effects. Whether it exists or not we need not ask. Grant to the full that it exists and Hume's analysis remains untouched. Deny on the other hand that it exists, and your denial cannot rest upon his analysis; it must have some other ground. The *nisus* in matter during one moment does not, if it exists, contain, or contain the prophecy of, the event that ensues or the new phase of *nisus* that ensues. Only experience tells us that muscular effort, for example, will be followed by a movement of matter or tend to be thus followed. The preceding exertion of "force" in nature would stand to the succeeding state of things precisely as in a "forceless" world one motion or collocation would stand to the next; so far, that is, as implication is concerned. When Hume denied "power" and "energy" it was only in the sense of something supposed to involve in its very idea the certainty of the effect. If we press with all our strength against a door and it gives way, its doing so was not contained or implied in our sensible pressure, even if taken with all the physical circumstances, and could not be predicted solely therefrom. Compelling a result is forcing it to be. But there is no case in which the result could conceivably be read out of the forces engaged (apart from the instruction we receive from experience), however we conceive them. Compelling is not implying.

Suppose even that energy is an entity that literally passes out of the cause into the effect and thence into new effects. Does this give us a cause that contains and can prophesy its effect? By no means. We could not know from an examination of its nature, however ideally complete, that the energy would continue to be energy or continue to be at all.

The phrase "operating principle" was used above to reflect a common impression: that the quasi-muscular "force" conceived to reside in matter contains within itself the explanatory secret of physical motion or change of motion, and that without it something would be lacking to the satisfaction of the intellect; there would be no real answer to the question Why? This is in so far reasonable as it is based on experience,—or on experience *plus* the natural human interpretation of it which attributes quasi-muscular "force" to nature; in so far reasonable as it is based on the fact that only by exerting muscular effort can we move matter, and on the belief that only by the equivalent of muscular exertion can matter move other matter or affect its motion. But it is an error to fancy that the exertion of such a force affords to the intellect, with respect to the origin of motion, any insight that must otherwise be lacking. The explanatory

plausibility of "force" comes from its being so familiar an antecedent of the effect, and from nothing else; the effect can be deduced from it no more than from a "forceless" motion. If there is another source for this special explanatory claim, could it be obligingly pointed out? Were a "forceless" motion as the antecedent equally familiar to the mind it would seem just as explanatory. In some cases, as we shall presently notice, it is equally familiar and seems in fact just as explanatory. It explains, that is, it assigns the cause, that is, it points to the sufficient occasion.

Here we see once again how deeply Hume's distinction cuts. Wherever it is held that there is a force, energy, agency, activity, efficacy, power, potency, potentiality, creativity, causative virtue, or operating principle, subsisting in the cause and insuring the effect; that we feel, perceive or in some sort experience this fact in the cause, or that we merely are obliged to impute it to the cause; in any case it is conceived as a *quality inhering in the cause*. Now the distinction must be drawn between this quality, whatever precisely it may be, as existing in the cause—this on the one hand—and, on the other, the fact that the event called the effect will ensue. They are two facts, not one; and any quality of the cause, however closely associated in our thinking with the sequel, is distinct from it and does not of itself imply it. There is a snare in these terms: by definition they presuppose that the later event will come (and two of them in mechanics mean this alone), but in the minds of the philosophers who make much of them they also are the name of a quality (however vaguely imagined) present in the cause; they thus stand for both of two distinct things; but these philosophers are prone to fancy that the presence of the one fatally implies the actual coming of the other and do not realise that the two are simply different facts. That is, they confound what we are here trying to put asunder; they are hazily conceiving that a quality in the cause *can* contain an implication of the effect. Hence the imperative need of drawing the distinction. The names given to the cause or to some quality of it, as above, may be for common language appropriate enough, may be expressive of its introductive rôle in the actual order of nature and of its *quale* as a *nisus*, or the like; it may feel in a very human sense "creative" because it is always connected with creation, with the bringing into being of new fact; nevertheless we have to recognise its quality, felt or conceived, as one thing, and the fact that the effect does ensue as another. The effort to roll up the latter in the former is natural and felicitous for ordinary

practical expression, it follows a law of language, but for philosophical analysis it is inept. It would be a mistake to say that potentiality or creativity do not exist; these are expressive words; something so called with imaginative fitness does or may exist. The thesis here is merely that though they may be conjoined by definition for our convenience, it is only by experience that we have learned that they are conjoined in fact. The truth is not: "Hume denies creativity"; it is that any conceivable creativity would be subject to Hume's law.

Those who fall back upon such words are looking for a cause within the cause. This is justified in one sense, futile in another; and unfortunately it is the futile quest which is associated with this set of words. The quest is justified in that experience shows us that objects and events are composed of smaller objects and events, and that the effects of the larger totals may be explained by (that they vary with) the character of their parts. Physics might justly be called a quest for causes within apparent causes (*pace* the critics who would extrude the word cause from science). But such a quest will find the inner cause sustaining no other relation to its sequel than the gross apparent cause seemed to do; the relation of a sufficient occasion to that which follows it. Look for a cause within the cause, if the inner one is to be the same sort of cause. But if the inner one is to be not an event nor its *quale*, but something else marvellously inhabiting an event, an elusive presence that is the only real cause because it is the whole productive power of the event, the soul of its efficacy—then we have passed into a romantic folklore that has its fine imaginative value but is of no aid in our analysis. By seeking a cause within the cause you cannot evade the nature of all causes. "The soul of its efficacy" would not be or possess any efficacy unless in the order of the universe the later event did in brute fact follow when it conceivably might not. These phrases, let us repeat, are not to be condemned at large, but as employed upon this problem they are a refusal to analyse. The essential service of Hume in the subject was that he gave the *causa occulta* its *coup de grâce*—though in philosophy alas! it does not know that it is dead.

"But all this argument," some one might interpose, "is vitiated by one easy assumption. You succumb to the temptation to take for granted that the whole nature and being of a thing or event is something presentable to the senses or to the sensuous imagination, that it could conceivably be given as a sensible datum, in a block as it were. Therein you overlook something that lies deep in the scientific conception of the world. You

overlook properties (in the sense which physical science now attaches to that term) which as inhering in a thing are precisely not picturable, which therefore elude the imagination, but which science none the less lays at the basis of its conceptions. We have here an ultimate term of thought. If we come to know a thing's properties, or let us say, more cautiously, if we could come to know them, then surely we could say, 'Since these are its properties it will in certain situations behave thus and thus and produce such and such effects.' And this would be finding from examination of a thing's nature that certain other concrete facts will come to be." The objection is taken up at this point, because the notion of properties, rendered radical, would perhaps identify them with the potential energies, together with the structure, of the material objects in question. But in truth I have already replied to it. I have said that we are alleged either to experience potentiality in the cause, or merely to impute it to the cause, and the comment in the latter case is the same as in the former. Is a "potentiality" (considered as a real attribute of an object, not this time of an event) in exactly the same state while latent as when in full operation? Is it in exactly the same state as its own realisation? Clearly not. Then the "latent" state is a different concrete fact from the realised state, is a fact of a different specific description, and the one cannot be extracted by deduction from the other, does not in itself prophesy the other. The suggestion that "property" is an ultimate term of thought may not be used as an instrument for injecting fog into the subject or a pretext for warning us away from too close a scrutiny. The term is analysable in any case to the extent that we may say it refers to something in the present, or something in the future, or both. But let us venture further and ask, What is a "property"? In the light of our distinction we see at once that the term is an eminently convenient device of language for gathering up the facts of a thing's future behaviour under various possible circumstances and attaching this hypothetical destiny to the thing as its present attribute. "But this behaviour will follow from the nature of the thing." Oh no, not from its present nature, severely taken. If water is boiling its present nature is to be boiling. When it is cold its present nature is to be cold. These are different natures and the one does not follow by deduction from the other, even taken with the surrounding state of things. The predicted behaviour will take place if the component parts of the thing and of other things behave as we expect. The future behaviour will be a set of sheer new facts. They are not contained in the

present fact at all. We anticipate them only from observation. It is a mere happy trick of language to gather them up in a term and seem to stow them in the present object. We do not infer the behaviour from the properties, we merely state the behaviour, after we have ascertained it from experience, in the form of properties.

"It does not follow from the nature of the thing." But in one sense of "nature," be it noted, it does follow from it. And that is the usual sense. The nature of a thing, above, has meant all its qualities and inner relations as they now actually are, the complete present description of it, so to speak. But this is a forced, technical limitation of the term, apt only for analytical purposes. Usually and normally it is used to include not only all this, but also the properties of the thing as just defined, the description of it as destined to behave thus and thus and call forth such and such behaviour under various circumstances. Since the future behaviour of a thing follows from its properties because its properties are nothing but the fact that such will be its behaviour, its future behaviour follows from its nature, for that includes its properties. These properties, if carelessly imagined as present *qualities* of the object, are however "not picturable" for a good reason; they are not there.

Bosanquet writes:—

"The basis of Induction is usually stated in some such formula as 'Same cause, same effect.' It is unnecessary for our present purpose to raise the questions connected with the converse formula, 'Same effect, same cause.' It is enough to understand the simplest truism of Identity, that a thing does what it is its nature to do under given conditions, and cannot do otherwise except by some change in the conditions; from which it follows, that if, in an alleged causal nexus, the alleged effect is sometimes absent while the alleged cause is present, *ceteris paribus*, it is impossible that the alleged cause should be the real cause of the effect in question. The principle is sound, beyond any doubt, as far as it goes. It is, in fact, nothing more than can be read off from the law of non-contradiction as formulated, for example, by Plato. The same thing cannot behave differently to the same thing in the same relation. If it seems to do so (Plato's condition "at the same time" is superfluous), you can infer that there is a difference in the supposed agent. The same, so far as it is concerned (i.e., if no condition is altered), produces the same; what produces something different, out of itself, is not the same. If this much is not to be assumed, we cannot treat anything as having an assignable nature. Truth ceases to have a meaning. Anything might behave anyhow."

A quite unconscious dexterity covers the gap in the argument. "Same cause, same effect"—of course; for the *word* cause is defined as meaning (or on any theory partly meaning) that which

will always be attended by the same sequel. If we call an event the cause of another, we are thereby asserting that the former, if repeated, will again be followed by the latter. This is an implication of the mere word. But do we know that the event is a cause; that it has a right to the name; that it *will* be followed by the latter; that is the question. Not from the nature of the event taken by itself, not from the law of non-contradiction; we can know it only from experience. We do not contradict ourselves if we say that a given event (not calling it a cause) might occur again and not be followed by the same phenomena. We may not believe that this would happen, we may infer from experience that it would not, but we cannot infer it—that is the point—from “the truism of Identity.” “Any thing might behave anyhow”—yes, so far as deductive logic can determine; that is precisely Hume’s thesis; deductive logic has nothing whatever to say on the subject. As regards what would actually happen we might agree with Bosanquet, though physical speculation is at present far from doing so. “A thing does what it is its nature to do under given conditions and cannot do otherwise except by some change in the conditions.” Here “does” may mean “causes” or “be a cause of,” in which case by definition the same effect must always follow, making the argument again verbal. Or it may mean “behaves.” Transcribing in this sense: “A thing behaves as it is its nature to do under given conditions and cannot behave otherwise,” etc. This version would throw the whole burden of the proof on the phrase “as it is its nature to do,” under which a confusion hides, as already seen. To say that a thing will behave as it is its nature to do, is to say that it will behave as we have found that it does behave, nothing more. “If in an alleged causal nexus the alleged effect is sometimes absent,” etc. Of course, but again the conclusion is verbal, resting solely on the definition of causal nexus. The conclusion is indeed read off from the law that we must not contradict our own definition. But it is not proved that an *event* or *group of circumstances* might not recur and what followed it before be absent. And when Bosanquet actually identifies the purely verbal conclusion with quite another, a conclusion about matter of fact and not merely about the proper bestowal of names, that “the same thing cannot behave differently to the same thing in the same relation” (meaning at another time and referring to the relation as it is at the outset of each occurrence) he falls into elementary fallacy. If we include “at the same time” the proposition becomes at once verbal, true, and irrelevant to our question. “The same, so far as it is

concerned (*i.e.*, if no condition is altered), produces the same." Once more, "produces" means (or includes in its meaning) "causes," and if a single such occurrence is really a causing, if "the same" is here a cause, then by definition it will always produce the same; but that the same will always be followed by the same is still unproved. In the whole paragraph he has taken no step to prove it, except that of invoking the thing's "nature." "An assignable nature" is in the circumstances a happy phrase; he is *assigning* to a thing, as its present nature, the facts of its future behaviour and the ensuing thereupon of future behaviour in other things, all of which is distinct from aught that now exists in that thing. "Truth ceases to have a meaning" is a fittingly vague sentence for the final phase of such an argument. Just so sadly has every attempt to base prediction on insight into the present broken down.

"But," it may be said, with a glance over all the foregoing, "do you actually mean that there is no reason in things for the activities they display, no ground in the being of the world for the operations or sequences that go on in it, that its history is nothing but a series of inexplicable happenings, of sourceless upstartings? Such an analysis utterly denies its rationality."

To put ourselves in a position to answer this question sufficiently we must turn from force and energy (which have involved us in all the last subject-matter) to consider necessity. With that preparation we shall confront the question in the concluding article.

1. Necessity and possibility are, of course, correlative. Necessity is the confinement of possibilities to one. The necessity of an event is the impossibility that aught else should happen at the time and place. We call an event possible when it presents itself to our mind and we know of nothing to prevent its occurring. We call it impossible when we believe that there is indeed something that will prevent its occurrence.

2. When mankind say that an effect will necessarily take place, they always conceive that it is made necessary by the nature of the cause. It is such as to entail this effect. If such and such expedients are brought to bear, says the workman in a difficulty, the desired result must follow, will have to follow, is bound to follow. The nature of the cause it is which is conceived to exclude other possibilities, to secure the one desired effect. If *that* factor is present, other effects are impossible.

3. Mankind clearly do not mean, however, that it is by implying the result that the nature of the cause makes it necessary. They are not hugging to themselves an ideal of deductive logic. It

is reserved to the tiny minority called philosophers to do that. The majority would require to be patiently practised in trains of thought quite foreign to them before they could even perceive the question at issue as to deductive consequence. Moreover, deductive necessity, having in truth no relevancy to actual effects, cannot be explicitly conceived by anybody as applying to them; and why should we charge the mass of mankind with insisting on vaguely and fallaciously conceiving it? The only inducement of the philosophers so to do is that very ideal of deductive satisfaction as the only complete intellectual peace, of which ideal the majority have never heard or dreamed, being entirely satisfied in the most anxious crises of their lives by a species of inference that has nothing to do with it.

4. What then do they mean when they think of one fact as by its special nature necessitating another? They refer to the recognised order of things, they mean that such is the unfailing march of events; that a fact of just this kind does in very truth follow as a matter of course upon a fact of just that kind. (This answer is incomplete; the next paragraph supplies the complement.) By necessary sequence we mean a sequence that will unconditionally take place. If *per impossible* the cause implied its effect, the effect would come as a matter of course. But if there is a recognised actual order of events, then also the effect comes as a matter of course. And its coming as a matter of course is the essential fact in the natural human idea of it as an effect. The difference between "This will happen" and "This must happen" is that, in the first one refers merely to the event itself (with a sufficient setting to fix the time intended) while in the latter one is mentally referring also to the presence of antecedents such as the event is sure to follow. Certain antecedents are regarded as decisive, that is, they are found a sure portent and precursor of the other facts in question.

5. However, in the words "cannot," "impossible," "must," there appears to be asserted something more than mere sequence. Now there are, as we know, certain terms that have valid and important meaning, yet have that meaning only from the point of view of the thinker's situation; the characters which they express are not resident in things regarded simply in themselves. Probability, for instance; there is no probability in things themselves; the term has proper significance, but only to a mind that is looking forward without complete knowledge. (Whether such characters have a subsistence independent of the mind as universals is a distinct question with which I do not here meddle.) Another instance is possibility; we call a thing possible, as we

have seen, when we contemplate it without knowing of anything to prevent its occurrence. This is not a condition of nature but a state of information of the mind. The same is true of impossibility. Here we do know of something to prevent the event's occurring, namely, something that will be followed, not by this, but by another event incompatible with it. But that which makes us say "impossible," "not possible," "not an event for which we must be prepared as not knowing whether it will happen or not," is a peculiarity of our own outlook. There is no impossibility in nature; there is only actuality. The impossible consists of removed possibles, which the intellect contemplates to deny. Nature does not contain the exclusion or rejection of anything: that is the mind's affair; nature contains only what is. Therefore necessity of occurrence also—being the impossibility of any other occurrence,—the fact that an event *cannot but* take place, that all other possibilities are removed,—is a character relative to the mind's business only; it is not sheer nature but second nature, if I may be suffered to use the phrase in a new sense. Thus it is that mere sequence in events themselves legitimately generates necessity in them as characterised by us. Just as there is no probability in things but only actuality (past, present, and future) so there is no necessity in things but only sequence; yet both terms, from the mind's point of view, have a valid and useful significance.

This is true of necessity in all its applications. When we say that one proposition necessarily follows from another, the intrinsic fact is that the first does in its veritable meaning imply the second. "Necessarily" means "There is no other way about it," "It could not fail to follow," and that thought arises only upon our taking notice of the alternative to reject it. Since necessity in this case also is a character that has reference to alternatives it has status only for the mind. Just as to describe one event as implying another in the deductive sense is to confuse the character of propositions with the character of things, so to conceive necessity as residing in objective or mathematical relations is to confuse the apt commentary of the intellect with the nature of things or of logic. So that if an effect were really implied in its cause the necessity of its coming would be a fact having status only for the mind, just as it is in the actual situation.

6. An unfailing order of events—an order in which "Water wets and fire burns"—in which, more widely and exactly speaking, there is such a thing as a sufficient occasion, an assemblage of circumstances that will always be followed by the same: I have

spoken as if we had knowledge of this. In fact experience gives us for it not a certainty but a high probability, so high that the chance of this order failing us may be treated for purposes of life and applied science as negligible. Another reason for treating it so is that we have no choice; so far as we regard all laws as doubtful we have no ground for a course of action or of inaction, only for the mental attitude of preparedness for the unexpected. To the consideration of experience as a source of evidence we come in the second article. Here it need only be said that the whole idea of cause is bound up with that of such a definite order, as the whole idea of necessity and of explanation is bound up with it. Causality is legality. Having noted once for all that the entire scheme of thought concerned with these terms is probable, not certain, we may go on to speak freely of that scheme, as before, without repeating at every turn this qualification. When we use the word "cause" or "must" or "why," we are taking that scheme for granted, as it is convenient to do.

7. It may plausibly be suggested that "must happen" carries with it always a dynamic idea in a special sense, that it always refers to compulsion by a major force, conceived in the quasi-muscular fashion. It does so often of course, and where it does we have only to remind ourselves that this is but one example of sequence without implication. If in every case "must" and "necessity" referred to such a force, they would be in no discord, as we saw, with Hume's analysis. But do they? Surely there are cases enough, mental and social causings, political effects, disturbances of the money market, embarrassment caused by a remark, grief precipitated by a sentence of news, shock due to the sight of an accident, and even many obviously physical causings, such as the turning on of electric light, where we do not think of any impressed force in this sense and should have to pause and exert the trained ingenuity of a scientific imagination to do so, and where we yet do emphatically say that the effect "must" follow. Yes, it may even be questioned how often, when workmen at a bit of ordinary construction or repair say that to use a certain means *must* bring at last the effect desired, the idea of such force comes to their minds. Often, to be sure, and yet the frequency with which it does so can be exaggerated. So it is seen that the whole essential basis of the idea of a necessary event in the form in which it prevails amongst mankind is the idea of an unfailing order.

8. It is often said that natural science does not undertake to be explanatory in a radical sense, but only descriptive. But to be explanatory even in a radical sense is to be descriptive

—descriptive of what is or was, and will be or would be. It is to describe certain sequences and to describe them as constant in Nature. To explain is to tell why. But every Why may be resolved into a How—how the thing occurs—and every How may be resolved into a What—what first, what next, what then. If after some disastrous accident we ask why the boat capsized, or how it happened, or what happened, we are asking the same question. To tell the What is to describe.

Expressing the same in other words : to explain an event is to show that it had to happen as it did. That means to exhibit it as the effect of a cause, in other words as a case of a law. Necessity is born of a law. Hence a law cannot be necessary, unless indeed it is born of another law. An ultimate law cannot be necessary. In each instance of a law the effect is necessary because the law is a fact. But there is no necessity that this general fact should exist, if we are speaking of an ultimate law. There is no reason to give why some other law might not have existed. Thus necessity, by its very nature, we see again, is born of mere sequence.

9. When we use the word "cause" or the phrase "must happen" we are taking for granted, as we saw, a scheme of thought based on laws, that is, uniformities of sequence. We are taking that scheme for granted, and yet ordinarily (be it never overlooked by philosophers) we have not that scheme in mind. Law, uniformity, "always" are in this subject sound ideas, but they are not present to us daily when we deal with causes. We do not bethink ourselves that the antecedent will always usher in a certain event, but we believe that it surely will do so in the particular instance. We do not take the trouble to make the generalisation. Why should we? What has it to do with the business in hand? The event must come, for certain antecedents are there. Here is this, so that must come after. The nature of a cause or sufficient occasion may equally well be expressed in the hypothetical as in the universal form : If you use a certain means, or if a certain event occurs, such and such phenomena will ensue. Or in the form of a particular inference from a particular fact. In these forms of thought there is no "all" nor even an "any" actually in the mind, though of course they could promptly be inferred. Do this and you will get that. With regard to each case we have at once a conviction as to the sequence, but without mental reference to the evidence that supports it or to the generalisation that stands or falls with it.

10. But the moment the question "Why" is pressed, the omitted ideas come to mind. "It will." "Why?" "It must."

"Why must it?" "We're using such and such means." "Well, why must it follow?" "It always does." To be sure, a mechanical reason with respect to the working of parts could often be given. But why do those parts work thus? In the last resort, "They always do." They will not fail to do so on this occasion because at no time do they fail. The assertion is double; it is not merely "They always have"; by its form it not only reports past experience but asserts a rule. It points to the rule as the ground of the necessity in the individual case and throws in past experience as evidence for the rule. Thus necessity, when asked for its ground, points to universality. This is in accord with the principle that a law (except when it follows from another law) is not necessary, but a particular effect exemplifying it is so. The "must" that we apply in daily life to the sequences in nature is usually, I repeat, applied without thought of "all cases," but depends none the less for support on a truth respecting all cases. We may say, indeed, that our "must" in ordinary speech is our handy substitute for a consciously universal proposition, that in this sphere necessity is in effect the focussing of universality upon the particular case.

(To be concluded.)

II.—THE PRINCIPLES OF DEMONSTRATIVE INDUCTION (I.)

BY C. D. BROAD.

SOME years ago I wrote two articles in *MIND* on *Induction and Probability*, and, more recently, in my presidential address to the Aristotelian Society I tried to state as fully and clearly as I could the present position of the logical theory of what Mr. Johnson calls "Problematic Induction." In the present paper I propose to do the same for what he calls "Demonstrative Induction." In the former undertaking I was greatly indebted to Mr. Keynes, and in this I am even more indebted to Mr. Johnson. All my raw material is contained in his work on *Logic*, and I can claim no more than to have beaten it into a more coherent shape than that in which he left it. I think that my approach to the subject by way of the notions of Necessary and Sufficient Conditions has certain advantages, and that I have been able to make some extensions of the theory. This must be my excuse for publishing a rather long and tedious essay on a somewhat hackneyed subject which has been treated so fully and so recently by a logician of Mr. Johnson's eminence.

1. DEFINITION OF "DEMONSTRATIVE INDUCTION."—A demonstrative induction is a mixed hypothetical syllogism of the form *Modus Ponendo Ponens* (i.e., if p then q , But p , Therefore q), in which the premises are of a certain form. The major premise must be either of the form (a) If *this* S is P then *all* S is P , or (b) If *at least one* S is P then *all* S is P . In the first case the minor premise must be of the form *This* (same) S is P . In the second case the minor premise must be either of the form *This* S is P , or of the form *At least one* S is P . (It is of course obvious that the former implies the latter, whilst the latter does not imply the former.) The conclusion is always of the form *All* S is P .

We can sum this up in words as follows. The major premise must be a hypothetical proposition, in which the consequent is a universal categorical, and the antecedent is either a singular or a particular categorical of the same quality and with the same

subject and predicate terms as the consequent. The minor premise must be the antecedent in the major if this be *singular*. If the antecedent in the major be *particular* the minor premise may be either this antecedent or may be a singular proposition with the same subject and predicate terms and the same quality as the antecedent. The conclusion is always the consequent in the major premise.

In the notation of *Principia Mathematica* the three forms of demonstrative induction may be symbolised as follows:—

$$\begin{array}{l} \phi a . \psi a : \supset : \phi x \supset_x \psi x \\ \phi a . \psi a \\ \therefore \phi x \supset_x \psi x. \end{array} \quad (\text{I.})$$

$$\begin{array}{l} (\exists x) . \phi x . \psi x : \supset : \phi x \supset_x \psi x \\ \phi a . \psi a \\ \therefore \phi x \supset_x \psi x. \end{array} \quad (\text{IIa.})$$

$$\begin{array}{l} (\exists x) . \phi x . \psi x : \supset : \phi x \supset_x \psi x \\ (\exists x) . \phi x . \psi x \\ \therefore \phi x \supset_x \psi x. \end{array} \quad (\text{IIb.})$$

An example would be: "If someone who sleeps in the dormitory has measles, then everyone who sleeps in the dormitory will have measles. But Jones sleeps in the dormitory and has measles. (Or, alternatively, Someone who sleeps in the dormitory has measles.) Therefore everyone who sleeps in the dormitory will have measles." This illustrates IIa. and IIb. The following would illustrate I.: "If the gas Hydrogen can be liquefied, then every gas can be liquefied. But the gas Hydrogen can be liquefied. Therefore every gas can be liquefied." I think it is worth while to note that when we use a major premise of this form we are generally taking an extreme instance (*e.g.*, Hydrogen, because it is the lightest and most "gassy" of all gases), and then arguing that if *even* this has a certain property all other members of the same class will *a fortiori* have it. Another example would be the premise: "If the philosopher X can detect no fallacy in this argument no philosopher will be able to detect a fallacy in it." We might be prepared to accept this premise on the grounds of the extreme acuteness of X. But we certainly should not be prepared to accept the premise: "If some philosopher or other can detect no fallacy in this argument then no philosopher will be able to detect a fallacy in it." For the philosopher Y might well rush in where X would fear to tread.

In all cases that we are likely to have to consider, the major premise of a demonstrative induction rests ultimately on a problematic induction. In all such cases it will only have a certain degree of probability. Consequently, although the conclusions of demonstrative inductions do follow of necessity from their premises, they are only probable, because one at least of the premises is only probable. It may happen that both the premises are only probable. Take, *e.g.*, Mr. Johnson's example about the atomic weight of Argon. The ultimate major premise is no doubt the proposition that if some sample of a chemical element has a certain atomic weight then all samples of that element will have that atomic weight. This is a problematic induction from an enormous number of chemical facts, and is only probable. (In fact, owing to the existence of Isotopes, it is not unconditionally true.) But one would also need the premise that Argon is a chemical element. This is again a problematic induction from a large number of chemical facts. And it is only probable.

The argument about Argon, when fully stated, would take the following form: (i) If some sample of a chemical element has a certain atomic weight, then all samples of that element will have that atomic weight. But Argon is a chemical element. Therefore if some sample of Argon has a certain atomic weight *W* all samples of Argon will have the atomic weight *W*. (This is an ordinary syllogism.) (ii) Therefore if some specimen of Argon has the atomic weight 40 all specimens of Argon will have the atomic weight 40. (This is a conclusion drawn by the Applicative Principle.) (iii) This specimen of Argon has atomic weight 40. Therefore all specimens of Argon will have atomic weight 40. (This is the demonstrative induction.) The empirical premises are three, *viz.*, the original generalisation about chemical elements, the proposition that Argon is an element, and the proposition that the atomic weight of this specimen of Argon is 40.

Now much the most important major premises for demonstrative inductions are provided by causal laws. It will therefore be necessary for us to consider next the question of Causal Laws.

2. CAUSAL LAWS.—The word "cause" is used very ambiguously in ordinary life and even in science. Sometimes it means a necessary, but it may be insufficient, condition (*e.g.*, "sparks cause fires"). Sometimes it means a sufficient, but it may be more than sufficient, condition or set of conditions (*e.g.*, "Falling from a cliff causes concussion"). Sometimes it means a set of conditions which are severally necessary and jointly sufficient. But, in any interpretation, it involves one or both of the notions

of "necessary" and "sufficient" condition. It is therefore essential to begin by defining these notions and proving the most important general propositions that are true about them.

There is one other preliminary remark to be made. There are two different types of causal law, a cruder and a more advanced. The cruder type merely asserts connexions between *determinable* characteristics. It just says that whenever such and such determinable characteristics are present such and such another determinable characteristic will be present. An example would be the law that cloven-footed animals chew the cud, or that rise of temperature causes bodies to expand. I shall call such laws "*Laws of Conjunction of Determinables.*" The more advanced type of law considers the determinate values of conjoined determinates. It gives a formula from which the determinate values of the effect-determinables can be calculated for every possible set of determinate values of the cause-determinables. An example would be the law for gases that $P = RT/V$. I will call such laws "*Laws of Correlated Variation of Determinates.*" In the early stages of any science the laws are of the first kind, and in many sciences they have never got beyond this stage, *e.g.*, in biology and psychology. But the ideal of every science is to advance from laws of the first kind to laws of the second kind. Now Mill's Methods of Agreement, Difference, and the Joint Method, are wholly concerned with the establishment of laws of conjunction of determinables. His Method of Concomitant Variations *ought* to have been concerned with the establishment of laws of correlated variation of determinates. But, since he talks of it as simply a weaker form of the Method of Difference, which we have to put up with when circumstances will not allow us to use that method, it is plain that he did not view it in this light. On the other hand, Mr. Johnson's Methods are definitely concerned with laws of correlated variation. They presuppose that laws of conjunction of determinables have already been established.

The order which I shall follow henceforth is this: (i) I shall deal with the notion of necessary and sufficient conditions wholly in terms of determinables. I shall then state Mill's Methods in strict logical form and show what each of them would really prove. (ii) I shall then pass to the notion of correlated variation of determinates, and explain Mr. Johnson's methods.

3. NECESSARY AND SUFFICIENT CONDITIONS.

(i) *Notation.*—The letters E, and C_1 , C_2 , etc., are to stand for determinable characteristics. I shall use C's to denote determining factors and E's to denote determined factors.

(ii) *Definitions*.—"C is a *sufficient condition* ('S.C.') of E" means "Everything that has C has E" (1).

"C is a *necessary condition* ('N.C.') of E" means "Everything that has E has C" (2).

" $C_1 \dots C_n$ is a *smallest sufficient condition* ('S.S.C.') of E" means that " $C_1 \dots C_n$ is a S.C. of E, and no selection of factors from $C_1 \dots C_n$ is a S.C. of E" (3).

" $C_1 \dots C_m$ is a *greatest necessary condition* ('G.N.C.') of E" means that " C_1 and C_2 and $\dots C_m$ are each a N.C. of E, but nothing outside this set is a N.C. of E" (4).

" $C_1 \dots C_n$ are *severally necessary and jointly sufficient* to produce E" means that " $C_1 \dots C_n$ is both a S.S.C. and a G.N.C. of E" (5).

(N.B.—I have represented the effect-determinable by the single letter E. This is not meant to imply that it really consists of a single determinable characteristic. In general, it will be complex, like the cause-determinable, and will be of the form $E_1 \dots E_m$. But in the propositions which I am going to prove in the next few pages the complexity of the effect-determinable is irrelevant, and so it is harmless and convenient to denote it by a single letter. Later on I shall prove a few propositions in which it is necessary to take explicit account of its internal complexity.)

(iii) *Postulates*.—(1) It is assumed that all the C-factors are capable of independent presence or absence. This involves (a) that none of them is either a conjunction or alternation of any of the others. (E.g., C_3 must not be the conjunctive characteristic C_1 -and- C_2 . Nor may it be the alternative characteristic C_1 -or- C_2 .) Again (b) no two of them must be related as red is to colour (for then the first could not occur without the second), or as red is to green (for then the two could not occur together). It is also necessary to assume that all combinations are *causally* possible. For otherwise we might have the two causal laws "Everything that has C_1C_2 has C_3 " and "Everything that has C_3 has E." In that case both C_1C_2 and C_3 would have to be counted as S.C.'s of E, since the law "Everything that has C_1C_2 has E" would follow as a logical consequence of these two other laws. This would obviously be inconvenient; we want to confine our attention to *ultimate* causal laws. Our present postulate may be summed up in the proposition that, if there be n cause-factors, it is assumed that all the $2^n - 1$ possible selections (including all taken together) are both logically and causally possible. This may be called the "*Postulate of Conjunctive Independence*."

(2) It is further assumed that every occurrence of any deter-

minable characteristic E has a S.S.C. This means that, whenever the characteristic E occurs, there is some set of characteristics (not necessarily the same in each case) such that the presence of this set in any substance carries with it the presence of E, whilst the presence of any selection from this set is consistent with the absence of E. This is the form which the Law of Universal Causation takes for the present purpose. We will call it "The Postulate of *Smallest Sufficient Conditions*."

(iv) *Propositions*.—(1) "If C be a S.C. of E, then any set of conditions which contains C as a factor will also be a S.C. of E."

Let such a set of conditions be denoted by CX.

Then : (a) All that has CX has C.

(b) All that has C has E. (Df. 1.)

Therefore all that has CX has E.

Therefore CX is a S.C. of E. (Df. 1.)

Q.E.D.

(2) "If $C_1 \dots C_m$ be a N.C. of E, then any set of conditions contained in $C_1 \dots C_m$ will also be a N.C. of E."

Consider, e.g., the selection $C_1 C_2$.

Then : (a) All that has $C_1 \dots C_m$ has $C_1 C_2$.

(b) All that has E has $C_1 \dots C_m$. (Df. 2.)

Therefore all that has E has $C_1 C_2$.

Therefore $C_1 C_2$ is a N.C. of E. (Df. 2.)

Q.E.D.

(3) "Any S.C. of E must contain all the N.C.'s of E."

Let X be a S.C. of E, and let Y be a N.C. of E.

Then : (a) All that has X has E. (Df. 1.)

(b) All that has E has Y. (Df. 2.)

Therefore all that has X has Y.

But all the C's are capable of independent presence or absence. (Postulate 1.) Hence this can be true only if X be of the form YZ.

Therefore any S.C. of E must contain as factors every N.C. of E, if E has any N.C.'s. Q.E.D.

(4) "E cannot have more than one G.N.C."

Let $C_1 \dots C_m$ be a G.N.C. of E. Then this set (a) contains *nothing but* N.C.'s of E (Prop. 2); and (b) contains *all* the N.C.'s of E. (Df. 4.)

Now any alternative set must either (a) contain some factor which is not contained in this one; or (b) contain no factor which is not contained in this one. In the first case it will contain some factors which are not N.C.'s of E. Therefore such a set could not be a G.N.C. of E. In the second case this set either coincides with $C_1 \dots C_m$ or is a selection from $C_1 \dots C_m$.

On the first alternative it does not differ from $C_1 \dots C_m$. On the second alternative it does not contain *all* the N.C.'s of E.

Therefore it could not be a G.N.C. of E.

Therefore E cannot have more than one G.N.C.

Q.E.D.

(5) "E can have a plurality of S.S.C.'s. These may be either entirely independent of each other, or they may partially overlap; but one cannot be wholly contained in the other."

Take, *e.g.*, C_1C_2 and $C_3C_4C_5$.

To say that C_1C_2 is a S.S.C. of E is to say that everything which has C_1C_2 has E; whilst C_1 can occur without E, and C_2 can occur without E. (Df. 3.)

To say that $C_3C_4C_5$ is a S.S.C. of E is to say that everything which has $C_3C_4C_5$ has E; whilst C_3C_4 can occur without E, and C_4C_5 can occur without E, and C_5C_3 can occur without E. (Df. 3.)

It is evident that the two sets of statements are logically independent of each other, and can both be true.

Now take C_1C_2 and C_2C_3 .

We have already stated what is meant by saying that C_1C_2 is a S.S.C. of E. To say that C_2C_3 is a S.S.C. of E means that everything which has C_2C_3 has E; whilst C_2 can occur without E, and C_3 can occur without E. If the two sets of statements be compared it will be seen that they are quite compatible with each other.

But it would be impossible, *e.g.*, for C_1C_2 and C_1 to be both of them S.S.C.'s of E. For, if C_1C_2 were a S.S.C., it would follow from Df. 3 that C_1 would not be a S.C. at all.

Q.E.D.

(6) "Any factor which is common to all the S.S.C.'s of E is a N.C. of E."

Let S_1 , S_2 , and S_3 be *all* the S.S.C.'s of E. And let C be a factor common to all of them.

Since every occurrence of E has a S.S.C. (Postulate 2), everything that has E has either S_1 or S_2 or S_3 .

But everything that has S_1 has C, and everything that has S_2 has C, and everything that has S_3 has C.

Therefore everything that has E has C.

Therefore C is a N.C. of E. (Df. 2.)

Q.E.D.

(7) "If E has *only one* S.S.C., it has also a G.N.C., and these two are identical. And so this set is severally necessary and jointly sufficient to produce E."

By Prop. 4 there cannot be more than one G.N.C. of E.

By Prop. 3 the S.S.C. of E must contain the G.N.C. of E.

By Prop. 6 any factor that is common to all the S.S.C.'s of E must be a N.C. of E. Now, since in the present case there is *only one* S.S.C. of E, *every* factor in it is common to all the S.S.C.'s of E.

Therefore every factor in the S.S.C. of E is a N.C. of E.

But we have already shown that every N.C. of E must be a factor in the S.S.C. of E.

Therefore the S.S.C. and the G.N.C. of E coincide.

Therefore this set of factors is severally necessary and jointly sufficient to produce E. Q.E.D.

(8) "If C be a S.C. of E_1 and also a S.C. of E_2 , then it will also be a S.C. of E_1E_2 . And the converse of this holds also."

The hypothesis is equivalent to the two propositions:—

All that has C has E_1 ; and

All that has C has E_2 . (Df. 1.)

Now these are together equivalent to the proposition: "All that has C has E_1E_2 ." And this is equivalent to the proposition: "C is a S.C. of E_1E_2 ." (Df. 1.) Q.E.D.

(9) "If C be a N.C. of either E_1 or E_2 , then it is a N.C. of E_1E_2 ."

If C be a N.C. of E_1 it follows from Df. 2 that all that has E_1 has C.

But all that has E_1E_2 has E_1 .

Therefore all that has E_1E_2 has C.

Therefore, by Df. 2, C is a N.C. of E_1E_2 .

In exactly the same way it can be shown that, if C be a N.C. of E_2 , it will be a N.C. of E_1E_2 .

Therefore, if C be a N.C. either of E_1 or of E_2 , it will be a N.C. of E_1E_2 . Q.E.D.

(10) "The converse of (9) is false. It is possible for C to be a N.C. of E_1E_2 without its being a N.C. of E_1 or a N.C. of E_2 ."

If C be a N.C. of E_1E_2 , then all that has E_1E_2 has C. (Df. 2.)

But this is quite compatible with there being some things which have E_1 without having C, or with there being some things which have E_2 without having C. (*E.g.*, all things that are black and human have woolly hair. But there are black things and there are human things which do not have woolly hair.)

So the truth of the proposition that C is a N.C. of E_1E_2 is compatible with the falsity of either or both the propositions that C is a N.C. of E_1 and that C is a N.C. of E_2 . Q.E.D.

(11) "If C_1C_2 be a S.C. of *each* of the effect-factors $E_1, E_2, \dots E_n$, and if it be a S.S.C. of *at least one* of them, then it will be a S.S.C. of the complex effect $E_1 \dots E_n$."

From Prop. 8 it follows at once that C_1C_2 will be a S.C. of $E_1 \dots E_n$. It is therefore only necessary to show that it will be a S.S.C.

Let us suppose, *e.g.*, that C_1C_2 is a S.S.C. of the factor E_1 . Then, from Df. 3, it follows that C_1 is not a S.C. of E_1 and that C_2 is not a S.C. of E_1 .

Now suppose, if possible, that C_1C_2 is not a S.S.C. of $E_1 \dots E_n$. We know that it is a S.C. of $E_1 \dots E_n$. If it be not a S.S.C., then either C_1 or C_2 must be a S.C. of $E_1 \dots E_n$. (Df. 3.) But, if so, then either C_1 or C_2 must be a S.C. of E_1 . (Prop. 8.) But we have seen above that neither C_1 nor C_2 can be a S.C. of E_1 .

Hence the supposition that C_1C_2 is not a S.S.C. of $E_1 \dots E_n$ is impossible. Q.E.D.

(12) "The converse of (11) is false. If C_1C_2 be a S.S.C. of $E_1 \dots E_n$, it will indeed be a S.C. of each of the factors; but it need not be a S.S.C. of any of the factors."

This is obvious. *E.g.*, C_1 might be sufficient to produce E_1 , though nothing less than C_1C_2 was sufficient to produce $E_1 \dots E_n$.

4. THE POPULAR-SCIENTIFIC NOTION OF "CAUSE" AND "EFFECT."—The notions which we have been defining and discussing above are those which emerge from the looser notions of "cause" and "effect," which are current in daily life and the sciences, when we try to make them precise and susceptible of logical manipulation. There are, however, certain points which must be cleared up before the exact relation between the logical and the popular-scientific notions can be seen.

(i) *The Time-factor.*—It might well be objected that the notion of temporal succession is an essential factor in the common view of cause and effect, and that this has disappeared in our account of necessary and sufficient conditions. The effect is conceived as something that begins at the same moment as the cause ends. And without this temporal distinction it would be impossible to distinguish effect from cause. All this is perfectly true, and it would be of great importance to make it quite explicit if one were dealing with the metaphysics, as distinct from the mere logical manipulation, of causation. But for the present purpose it may be met by the following remark about our notation. We must think of some of our C's as being really of the complex form "being characterised by \mathfrak{C} up to the moment t ," and of some of our E's as being really of the complex form "beginning to be characterised by \mathfrak{E} at the moment t ."

(ii) *Transeunt Causation.*—A second highly plausible objection would be the following. In our exposition of necessary and

sufficient conditions we have always talked of a single continuant, and have supposed that the effect-characteristics and the cause-characteristics occur in the same continuant. But in fact most causation is transeunt, *i.e.*, the cause-event takes place in one continuant and the effect-event in another. This, again, is perfectly true, and very important in any attempt at an analysis of causation for metaphysical purposes. The usual kind of causal law does in fact take roughly the following form: "If a continuant having the properties P is in the state S_1 at a moment t and it then comes into the relation R to a continuant which has the properties P' and is in the state S'_1 , the former continuant will begin to be in the state S_2 and the latter in the state S'_2 ." *E.g.*, "If a hard massive body moving in a certain direction and with a certain velocity at a certain moment comes at that moment into contact with a soft inelastic body at rest, the motion of the former body will begin to change and a dint will develop in the latter body."

For mere purposes of logical manipulation, however, all this can be symbolised as changes in the characteristics of the first continuant. We shall have to remember that some of our C 's and some of our E 's stand for relational properties of a very complex kind, involving relations to other continuants. Thus, in the example one of our C 's will be the characteristic of "Coming into contact at t with a soft inelastic resting body." And one of our E 's will be the characteristic of "Having been in contact at t with the same body beginning to develop a dint." All this is purely a matter of verbal and notational convenience. It has no philosophical significance. But it is harmless so long as we remember that our innocent-looking C 's and E 's stand, not just for simple qualities, but for extremely complex relational properties of the various kinds described above.

(iii) *Negative Factors*.—It must be clearly understood that some of the C 's and some of the E 's may stand for negative characteristics, *i.e.*, for the absence of certain positive characteristics. Negative conditions may be just as important as positive ones. *E.g.*, there is no general law about the effect of heat on oxygen. If the oxygen be free from contact with other gases it merely expands when heated. If it be mixed with a sufficient proportion of hydrogen it explodes. Thus the negative condition "in absence of hydrogen" is an essential factor when the effect to be considered is the expansion of oxygen.

5. PLURALITY OF CAUSES AND EFFECTS.

(i) *Total Cause and Total Effect*.—Before we can discuss whether plurality of causes or of effects is logically possible we must

define the notions of "total cause" and "total effect." The definition is as follows:—

" $C_1 \dots C_n$ stands to $E_1 \dots E_m$ in the relation of *total cause to total effect*" means that " $C_1 \dots C_n$ is a S.S.C. of $E_1 \dots E_m$, and it is not a S.C. of any characteristic outside the set $E_1 \dots E_m$." (Df. 6.)

It will be seen that this definition is equivalent to the conjunction of the following three propositions, one of which is affirmative and the other two negative:—

- (a) Any occurrence of $C_1 \dots C_n$ is also an occurrence of $E_1 \dots E_m$.
- (b) There is no selection of factors from $C_1 \dots C_n$ such that every occurrence of it is also an occurrence of $E_1 \dots E_m$.
- (c) There is no factor outside $E_1 \dots E_m$ such that every occurrence of $C_1 \dots C_n$ is also an occurrence of it.

(ii) *Plurality of Causes*.—With this definition it is logically possible for *several* different sets of factors to stand to *one and the same* set of factors in the relation of total cause to total effect. For we have proved in Prop. 5 that one and the same E can have a plurality of different S.S.C.'s. We also showed there that the various S.S.C.'s may either have no factor in common or may partially overlap, but that one cannot be wholly included in another. We also showed in Prop. 6 that any factor which is common to all possible S.S.C.'s of a given E is a N.C. of that E . It is, of course, quite possible for an effect to have *no* necessary conditions. For if it has two S.S.C.'s which have no factor in common, it cannot possibly have a N.C. On the other hand (Prop. 7), if an effect has only one S.S.C. this is also the G.N.C. of the effect. So, when there is no plurality of causes, the total cause of a given total effect is a set of factors which are severally necessary and jointly sufficient to produce the effect.

Thus our definitions allow the possibility of a plurality of *total causes* for one and the same *total effect*. Whether there actually is such plurality in nature, or whether the appearance of it is always due to our partial ignorance or inadequate analysis, is a question into which I shall not enter here. Of course, even if a given total effect does have a plurality of total causes, *each particular occurrence* of this total effect will be determined by the occurrence of one and only one of these total causes. The plurality will show itself in the fact that some occurrences of the total effect will be determined by occurrences of one of the total causes, whilst other occurrences of the total effect will be determined by occurrences of another of the total causes.

(iii) *Plurality of Effects*.—It is plain from Df. 6 that a given total cause could not have more than one total effect. Thus plurality of total effects is ruled out by our definitions.

6. FORMAL STATEMENT OF MILL'S METHODS.—We are now in a position to deal with Mill's Methods of Agreement and Difference. Mill never clearly defined what he meant by "cause" or by "effect," and he never clearly stated what suppressed premises, if any, were needed by his Methods. We shall now be able to see exactly in what sense "cause" and "effect" are used in each application of each Method; what assumptions are tacitly made; and what bearing the question of "plurality of causes" has on the validity of each application of each Method. Mill made two applications of each Method, *viz.*, to find "the effect of a given cause" and to find "the cause of a given effect." We have therefore in all four cases to consider:

(i) *Method of Agreement*.—(a) *To find the "effect" of A.*

The premises are:—

All ABC is *abc*; and

All ADE is *ade*.

The argument should then run as follows:—

A is not a S.C. of *bc*; for in the second case A occurs without *bc*. It is assumed that A is a S.C. of *something in abc*. Therefore it must be a sufficient condition of *a*.

Thus, the suppressed premise is that A is a S.C. of something or other in *abc*. And the sense in which it is proved that the effect of A is *a* is that it is *a* of which A is a S.C.

(b) *To find the "cause" of a.*

The premises are as before.

The argument should run as follows:—

From the two premises it follows that both ABC and ADE are S.C.'s of *a*. But every S.C. of *a* must contain all the N.C.'s of *a*. (Prop. 3.)

Therefore, if *a* has a N.C. at all, it must be or be contained in the common part of the two S.C.'s of *a*.

But the only common part is A.

Therefore, if *a* has a N.C. at all, either A itself or some part of A must be a N.C. of *a*.

Thus the sense in which it is proved that the cause of *a* is A or some part of A is that if *a* has a N.C. at all then it is A or some part of A which is its N.C.

Mill's contention that, in this application, the Method of Agreement is rendered uncertain by the possibility of Plurality

of Causes is true, and has the following meaning. If it be admitted that *a* may have more than one S.S.C. it is possible that it may have no N.C. at all. In fact, this will be the case if there is no factor common to all its S.S.C.'s. Thus, we cannot draw the categorical conclusion that the N.C. of *a* is or is contained in A unless we are given the additional premise: "*a* has either only one S.S.C., or, if it has several, there is a factor common to all of them."

(ii) *Method of Difference.*—(a) To find the "effect" of A.

The premises are:—

All ABC is *abc*; and

All (non-A)BC is (non-*a*)*bc*.

The argument should run as follows:—

A is not a N.C. of *bc*, for in the second case *bc* occurs without A.

It is assumed that A is a N.C. of *something* in *abc*.

Therefore A must be a N.C. of *a*.

Thus the suppressed premise is that A is a N.C. of *something* in *abc*. And the sense in which it is proved that the effect of A is *a* is that it is *a* of which A is a N.C.

(b) To find the "cause" of *a*.

The premises are as before.

The argument should run as follows:—

It follows from the second premise that All (non-A)BC is non-*a*.

Therefore, by contraposition, All *a* is non-[(non-A)BC].

Therefore, All *a* is either A or non-(BC).

Therefore, All *a* which is BC is A.

This may be stated in the form: "In presence of BC, A is necessary to produce *a*."

Now, the first premise could be put in the form: "In presence of BC, A is sufficient to produce *a*."

Combining these, we reach the final conclusion: "In presence of BC, A is necessary and sufficient to produce *a*."

We have no right to conclude that A would be either necessary or sufficient in the absence of BC. In the presence of a suitable mixture of hydrogen and oxygen a spark is both necessary and sufficient to produce an explosion with the formation of water. But it is not sufficient in the absence of either of the two gases. Again, when a person is in good general health, prolonged and concentrated exposure to infection is necessary and sufficient to give him a cold. But when he is in bad general health it is

not necessary that the exposure should be either prolonged or concentrated.

Thus Mill has no right to draw the unqualified conclusion that *A* is the cause of *a*, either in the sense of necessary or in the sense of sufficient condition. But he is justified in concluding that, in presence of *BC*, *A* is the cause of *a*, in the sense of being necessary and sufficient to produce *a*.

(iii) *The Joint Method*.—Mill's Joint Method is suggested as a method by which we may find the "cause" of *a* in cases where the Method of Difference cannot be used, and where the Method of Agreement is rendered untrustworthy by the possibility of Plurality of Causes.

It consists of two parts. The first is an ordinary application of the Method of Agreement. From this we reach the conclusion that, unless *a* has a plurality of S.S.C.'s with no factor common to all of them, *A* or some part of *A* is a N.C. of *a*. But, owing to the possibility of plurality of causes, it remains possible that *A* may be irrelevant to *a*. It may be, *e.g.*, that *BC* is a S.S.C. of *a* in the first case, and that *DE* is a S.S.C. of *a* in the second case, and therefore that *a* has no N.C. at all. The second part of the Joint Method is supposed to state conditions under which this possibility might be rejected. It is as follows. We are to look for a pair of instances which agree in *no* respect, positive or negative, except that *A* and *a* are absent from both of them. It is alleged by Mill that, if we find such a pair of instances, we can conclude with certainty that the "cause" of *a* is *A*.

It is, of course, quite plain that, even if the method were logically unimpeachable, it would be perfectly useless in practice. Any pair of instances that we could possibly find would agree in innumerable *negative* characteristics beside the absence of *A* and the absence of *a*. But is the argument logically sound even if premises of the required kind could be found?

It would run as follows. Since our two instances are to agree in *no* respect, positive or negative, except the absence of *A* and of *a*, *BC* cannot be absent in both of them. Therefore *BC* must be present in one of them. But *a* is absent in both of them. Therefore, in one of them *BC* is present without *a* being present. Therefore *BC* cannot be a S.C. of *a*. But, from the first part of the method, we know that *ABC* is a S.C. of *a*. A precisely similar argument would show that *DE* cannot be a S.C. of *a*. And, from the first part of the method, we know that *ADE* is a S.C. of *a*. Mill thinks that we can conclude that *A* is a N.C. of *a*. This, however, is a mistake. All that we can conclude is that, in presence of *BC* or *DE*, *A* is a N.C. of *a*. It remains quite

possible that there is another S.S.C. of *a*, e.g., XYZ, which does not contain A at all. And, in that case, A could not be a N.C., without qualification, of *a*. E.g., a certain kind of soil, when treated with lime, always yields good crops; and, when lime is absent, good crops are absent on this soil. This proves that the presence of lime is a necessary condition for getting good crops *with this kind of soil*. But it does not prove that the presence of lime is a necessary condition, without qualification, for getting good crops. With other kinds of soil it might be unnecessary or positively harmful.

There is, however, a perfectly sensible method of argument, which is not Mill's, but which might fairly be called the *Joint Method*. The first part of it would be to take a large number of sets of characteristics, such that each set contains A and that in other respects they are as unlike each other as possible. One would try to arrange that A should be the only characteristic common to *all* of them, though it might be impossible to arrange that any *two* of them had only A in common. Suppose it were found that every occurrence of each of these sets was also an occurrence of *a*. Then there would be a strong presumption, though never a rigid proof, that A was a S.C. of *a*. The alternative would be that *a* had an enormous number of alternative S.S.C.'s. The second part of the method would be to take a large number of sets of characteristics, such that each set *lacks* A, and that in other respects they are as unlike each other as possible. One would try to arrange that non-A should be the only characteristic common to *all* of them, though it might be impossible to arrange that any *two* of them had only non-A in common. Suppose it were found that every occurrence of each of these sets was also characterised by the *absence* of *a*. Then there would be a strong presumption, though never a rigid proof, that non-A was a S.C. of non-*a*. It would then follow by contraposition that A was a N.C. of *a*. Thus the combination of the two sets of observations would make it probable that A is a necessary and sufficient condition of *a*. The argument is, of course, greatly strengthened if the characteristics other than A and *a* which occur among the sets of the first series are, as nearly as may be, the same as the characteristics other than non-A and non-*a* which occur among the sets of the second series. Thus, as Mr. Johnson has pointed out, the various sets of the same series should differ as much as possible in all respects except the one under investigation; whilst the two series, as wholes, should agree as much as possible in all respects except the one under investigation. A good example would be provided by the

empirical arguments which lead to the conclusion that the property of having an asymmetrical molecular structure is a necessary and sufficient condition of the property of rotating the plane of polarisation of plane-polarised light. As Mill's own Joint Method is both useless and invalid, the name of "Joint Method" might be reserved in future for the above important and legitimate, though not absolutely conclusive, type of inductive argument.

(To be concluded.)

III.—IS THE TRANSCENDENTAL DEDUCTION A PATCHWORK ?

BY B. LUND YATES.

VAIHINGER's hypothesis as to the composition of the *Critique of Pure Reason* (1st edition) has been subjected to severe criticism on the Continent by such writers as H. C. Birven (*Immanuel Kant's Transzendente Deduktion*, Berlin, 1913: also published as a supplement to *Kantstudien*). But it has nevertheless been accepted as reliable by the leading Kantian 'authorities' in this country. So Prof. Paton's attack upon it, in his address of the above title (*Aristotelian Society*, 17th March) raises a somewhat new problem for English students of Kant. The issue is not altogether a trivial one, since it involves, on Vaihinger's part, a claim to reconstruct Kant's mental history during the important years 1771-81, while his English followers put forward the same hypothesis as a basis for a re-interpretation of the *Critique*. It will therefore perhaps be not out of place here to consider some of the principles at stake, from a point of view slightly different from that adopted by Prof. Paton.

I.

The first problem which we have to face arises from the arrangement of the text. For the *Critique* as a whole, and more especially the *Transcendental Deduction*, presents us with an argument that is by no means straightforward. Kant repeats himself time and again: he makes statements at some points in the argument which only become comprehensible when we have read passages which occur considerably later in the book: and sometimes he suddenly begins to discuss a new topic without any warning to the reader that he has 'changed the subject of conversation'. It has seemed to Adickes and Vaihinger that difficulties of this kind require special explanation, over and above the admitted complexity of the subject.

It is, however, not at all certain that any special explanation is necessary. For the mal-arrangement of the *Critique* is perhaps

an outstanding, but certainly a typical, example of Kant's normal method of exposition. Vaihinger himself remarks (*Die Transc. Ded. der Kat.*, Halle, 1902: 1st para.) that, in Kant's manuscripts, "we find, broadly speaking, a number of single shorter or longer sentences, in which Kant makes attempt after attempt to master some recalcitrant topic, and in these renewed attempts he hardly ever takes into consideration his own previous expositions of the subject". And, even in a work intended to be a comparatively popular introduction to his main ethical teaching, Kant falls into much the same faults of construction as we find in the *Critique* (though here, of course, the faults are not so glaring).¹ If, then, Kant cannot even give us a straightforward argument in a book where he has made every effort to be lucid, we really do not need any special hypothesis to explain his clumsiness or his rambling arguments in a work where, as he himself states, he gave "the closest attention to the content," but "little thought to the exposition or its ease of comprehension to the reader" (Letter to Mendelssohn, 16/8/83, quoted in Kemp Smith, *Commentary*, 2nd edition, p. xix).

However, if it is still insisted that we must have something more to render the arrangement of the *Critique* intelligible, Adickes has a plausible suggestion to offer:—that Kant, during the four or five months which he says he devoted to his final draft of the *Critique*, first wrote out a 'skeleton' of the whole work, and then added passages here and there, to introduce considerations which his 'skeleton' argument had omitted to mention or sufficiently to emphasise. I call this hypothesis 'plausible,' because it only supposes Kant to have followed, rather clumsily, no doubt, a practice which is common to many other writers. And Borowski tells us that this was Kant's usual method of composition.

Adickes' theory, moreover, is 'commensurate' with the facts it has to explain in a manner in which Vaihinger's theory is not. Vaihinger seems to have regarded his own hypothesis as merely

¹ In the *Grundlegung* we find at least two separate 'deductions' of the first formula of the Categorical Imperative: they are partly, but not wholly, repetitions of the same argument: and they occur in separate sections of the treatise. We have also three completely distinct formulations of the Imperative (not to mention minor variants), which, Kant goes on to maintain, mean really the same thing, and which he tries to relate by means of various expediences of 'architectonic'. Above all, what is perhaps the most important link in the 'transition' from the first to the second formulation of the Imperative, does not receive any mention in the main body of the text, being stated for the first time in the recapitulation of the whole argument which occurs near the end of the Second Section.

a fresh application of Adickes' suggestion. But we shall quite misunderstand the situation if we do not notice the very great difference between the two theories—a difference which seems to have been wide enough to prevent Adickes from accepting his successor's doctrine in place of his own. It is not merely that Vaihinger supposes the 'joints' in the text to occur at somewhat different places from those selected by Adickes. The whole scope of the two hypotheses differs, as can be seen from the fact that Adickes' distinction between 'earlier' and 'later' passages falls virtually *within* the last of the ten or more years covered by Vaihinger's various "phases": so that a manuscript written in Adickes' central period would almost be dubbed an 'afterthought' by Vaihinger.

Now it is clear that facts which might indicate that a portion of the text was probably written a few weeks (or even months) after its neighbour within the period immediately preceding publication, may very well be quite unsuited to suggest that the passage in question was written and laid aside several years previously. To show my meaning, let me quote a rather trivial example from Prof. Kemp Smith (*Comm.*, p. 222): The fact that a passage "makes a fresh start" and "stands in no necessary relation to any preceding section" is not at all surprising in view of the incoherencies in Kant's other writings: it may, nevertheless, possibly indicate that some interruption, sufficient to break Kant's train of thought (*e.g.*, a week's holiday), occurred between the composition of this passage and the completion of its immediate predecessor in the text. But this fact is most emphatically *not* evidence (and Prof. Kemp Smith does not make it quite clear whether he intends it as evidence) which supports the conclusion that the second passage must have been written in 1776-78 while the first was written in 1773-75. Certainly the "fresh start" is *compatible* with Vaihinger's hypothesis: but this is no *evidence*, since it is also compatible with various other explanations, Adickes' suggestion being one of them. Facts of this kind do not add to the probability of a theory of Vaihinger's dimensions: and to use such a hypothesis to explain them, when simpler explanations are to hand, would be like postulating an earthquake to account for a fall of cliff along a stretch of eroded sea-coast.

II.

There is, however, a more important characteristic of the *Critique* that seems to demand an explanation: namely, the

inconsistencies of its doctrine (as opposed to the disorderliness of its text). Unless I have misread him, *this* problem is the one which it was Vaihinger's ultimate purpose to solve. And, since it is really a distinct problem, requiring a different kind of treatment, it is to be regretted that Vaihinger conceived his own contribution as a mere extension of Adickes' hypothesis. For, it seems, because of this misconception Vaihinger felt himself called upon to prove that certain parts of the *Critique* were early *manuscripts*, in order to establish—what I cannot but suppose to have been his ultimate purpose—that they embody some of Kant's early *doctrines*.

Yet once more it is doubtful whether there is anything that really needs an explanation. Prof. Paton has argued very ably that the majority of the apparent inconsistencies in the Deduction are simply due to Vaihinger's misinterpretation of the text: and certainly, after reading the latter's essay on the Deduction, one is left with the uncomfortable suspicion that he has gone out of his way to find contradictions and incoherencies in Kant. Still, it might perhaps be replied from the other side that there is a limit beyond which one must not push the suggestion that what reads like an argument and was probably intended as an argument, therefore must *be* an argument. And there does seem to remain at least a possibility that certain real divergences of doctrine may be left unresolved, even when we have given Kant that benefit of the doubt to which he is entitled. But, since to settle the matter it would be necessary to compare the text, sentence by sentence, with Vaihinger's interpretation, let us here accept his contention for the sake of the argument, and see what legitimately follows from it.

Vaihinger's next step is certainly more plausible. With the aid of the few dated manuscripts which were accessible to him, and by considering what we may call the relative 'crudity' or 'maturity' of the conflicting doctrines in the *Critique* itself which he claims to have isolated, he arranges these doctrines in the order in which he supposes Kant to have invented them. No doubt, this was mainly a matter of shrewd guesswork and, as we shall see later, there is reason for believing that it was no more accurate than guesswork can hope to be: but Vaihinger's "detective genius" seems to have saved him from many of the mistakes which he might otherwise have made, and his procedure was not by any means absurd, even if it was very hazardous.

Moreover, there is no decisive objection to his suggestion that Kant may have worked from notes written in previous years when he was composing his final draft of the *Critique*, and that

perhaps he actually transcribed or inserted passages from old manuscripts into his "fair copy". So we may provisionally accept this hypothesis, although we must remember that it is not a very probable one, in view of the evidence as to Kant's method of composition which can be found in his manuscripts. For in these fragments,—the only papers that we have from Kant's own pen,—the philosopher (as Vaihinger himself remarks in the passage quoted above) "*hardly ever* takes into consideration his own previous expositions" of the subject in hand.

It must therefore be admitted, I think, that each step in Vaihinger's argument, so far as we have followed it at present, has a considerable element of doubt attached to it. But rather than be accused of unfairness to him, I propose to accept his premisses: for it is the conclusion which he draws from them, that seems to me to be the most objectionable part of his theory.

III.

Let us be quite clear as to the situation. We are supposed to have established some sort of correspondence between Kant's doctrines in the *Critique* and his teaching in earlier phases of the period 1771-81, and to have rendered it probable that he used old notes when composing the definitive text of the first edition. We are now to use these facts to explain certain (supposed) inconsistencies in the *Transcendental Deduction*.

Surprisingly enough, Vaihinger would appear to have overlooked the fact that our explanation, if it is to be an explanation at all, must be directed towards Kant's state of mind on the eve of publication, rather than his outlook in, say, 1772 or 1776. For even if we discover that Kant held one of the conflicting doctrines of the *Critique* in 1772, and another in 1776, we still need to explain what induced him to publish them both in 1781. Yet, so far as I understand him, Vaihinger makes no serious attempt to answer this question, although his theory, as I hope now to show, implies a most improbable account of Kant's attitude to the text and the doctrines which he decided to place before the public.

For there would seem to be at least two comparatively plausible explanations of the situation, both of which seem to be ruled out by Vaihinger's procedure. The first of these, to put it bluntly, is that Kant published doctrines invented by him at an early date, simply because he still believed them. Or in other words, since these earlier doctrines had been *historically* the starting point or basis upon which he had built his later teaching, Kant

(at least in 1781) did not notice that they were far from being the *logical* foundation of his maturer doctrines, and that they were undermined or even contradicted by his later teaching. In consequence he found it both permissible and desirable to publish them in the first edition of the *Critique*.¹

The reader will no doubt recognise in the above explanation something which is at least akin to suggestions put forward by Vaihinger's successor, Prof. Kemp Smith. But it must be carefully noted that, if we take these suggestions seriously, we at once deny all significance to the question of the date of the manuscripts included in the *Critique*, and also rule out any possibility of isolating and dating these manuscripts (as Vaihinger tentatively claims to do).

We deny the significance of the question, because our explanation of the inconsistencies in the *Critique* will depend solely upon Kant's state of mind in 1780-1, and not at all upon the date of the papers he sent to the printers. On the one hand, if Kant believed what he published, it would be quite possible for him to have written it at the time of publication. And, on the other hand, the discovery that he had actually written some passages several years before would in no way shake our hypothesis. (It is true, for example, that this article which I am now writing, is little more than an amplification of some pages I wrote two or three years ago: but I am using those old notes, because they still express my convictions in this matter. If

¹ I consider this a more or less plausible hypothesis, for two reasons. The first is that Kant often seems to be quite unconscious of the fact that a doctrine which he puts forward as a development of some earlier teaching, really is quite inconsistent with it. I need only refer the reader to the relations between the two later *Critiques* and the *Critique of Pure Reason*, in order to convince him of this tendency in Kant's thought. Secondly, this hypothesis finds confirmation in Kant's own remarks. In a note to the Preface of his *Metaphysische Anfangsgründe der Naturwissenschaft* (1786) Kant once more distinguishes his 'Objective Deduction' (the proof that experience is only possible because of the categories) from his 'Subjective Deduction' (the solution of the problem *how* experience becomes possible by these means). Then, dealing with the Subjective Deduction, the inconsistencies in which Vaihinger has used as his only 'clue,' Kant says, "The obscurity of my previous treatment of this part of the Deduction—an obscurity which I cannot deny—must be attributed to the usual fortune of the Understanding in its investigations, namely that as a rule the shortest way is not the first to be discovered: therefore"—since this problem "has great importance, although the main argument stands without it, and since, as I now see, its solution is equally clear"—"I shall take the next opportunity to get rid of that obscurity". Kant himself, then, in so far as he admits an obscurity in his doctrine at all, attributes it to a lack of insight rather than to any other cause.

Kant *did* use older manuscripts, very probably he did so for the same reason as in my own case.)

We also rule out the possibility of isolating and dating the manuscripts of the *Critique*. For unless we believe that Kant *no longer held* in 1781 what we know that he held in 1772-75 (say), we have absolutely no grounds for asserting that he could not have *written* in 1781 a passage expressing much the same doctrine "without reference to his older notes" (Vaihinger, *op. cit.*, p. 23). Therefore Vaihinger's assertion "that the Transcendental Deduction of the First Edition is definitely not by any means a unitary exposition, but one that is very loosely put together out of . . . statements from different periods of time" (*op. cit.*, p. 46) is *only* consistent with some explanation of their inclusion in the *Critique* that presupposes Kant to have *discarded* the doctrines in question some years before he published them. (I am almost ashamed to labour this point: but it has to be emphasised because all the writers who maintain that Vaihinger's "minute scrutiny has placed beyond a doubt that the *Critique* is the merest patchwork or mosaic of scraps" (Ward, *A Study of Kant*, p. 41) seem to overlook this important implication of their and Vaihinger's theory).

There was, of course, no evidence available in Vaihinger's time that could have supported this assumption which he tacitly makes. For the one dated manuscript of the period 1780-81 was far too short to show Kant's views on doctrines not mentioned in it. And the evidence which Adickes has more recently brought to light (see below, p. 329) does not justify Vaihinger's assumption either. Indeed, the only possible argument in favour of it, would be to the effect that the inconsistencies in the *Critique* are so glaring that Kant could not conceivably have believed all its doctrines, except at epochs separated from one another by a considerable lapse of time. And this argument is worthless. For, in the first place, the contradictions upon which Vaihinger bases his argument all occur in connexion with a topic which Kant did not regard as essential to his main position and into which, as we have seen, he admits that he had not clear insight in 1781: and therefore the contradictions most emphasised by Vaihinger are precisely those which were most likely to have reflected a genuine incoherency in Kant's outlook of 1781. Secondly, any argument based upon the inconceivability of contradictions in Kant's thought must fall to the ground in view of what we know and what Vaihinger's supporters have themselves urged as to Kant's unwillingness "to sacrifice insight to consistency" (Kemp Smith, *Comm.*, p. xxiii). And, finally, the fact that Kant was

prepared to include the doctrines in question in his authoritative exposition of the Critical Philosophy is the strongest possible argument that he still believed in them. Consequently, Vaihinger's claim to prove that the *Critique* is partly composed of early manuscripts, turns out to be based upon an assumption which we have no grounds for accepting and good reasons for rejecting.

Moreover, Vaihinger's "Patchwork" hypothesis does not by itself explain the presence of these contradictions in the Deduction. For we still have to show how Kant could have been induced to use these supposed early manuscripts if they no longer expressed his real convictions. Vaihinger himself does not attempt this explanation. But the suggestions put forward by his successors, debarred as they were from using the simple explanation that Kant meant what he said, are an interesting commentary upon the whole hypothesis. Prof. Kemp Smith, for instance, argues that "Kant, it would almost seem, objected to nothing so much as the sacrifice of an argument once committed to paper" and therefore "insisted" upon inserting these early arguments "no matter at what cost of repetition or even confusion" (*Comm.*, p. xxi). And yet Kant had not insisted upon inserting several arguments which had not only been committed to paper, but also consecrated in the print of earlier publications: and, as we have seen above, the moment he had found a simpler way of expressing his views, he took "the next opportunity" (i.e., the 2nd Edition) to excise some of the arguments to which Prof. Kemp Smith is here referring. Dr. Ward, again, seems to justify his position, by implying that Kant was "slovenly" and "in a hurry" (*A Study of Kant*, p. 41) when he composed the final draft of the *Critique*: and Prof. Kemp Smith seems at times to adopt the same explanation (*Comm.*, pp. xix ff.). But it should be noted that Kant himself, as against his apologists and in the letter to Mendelssohn upon which they base this explanation of the contradictions in the doctrine of the *Critique*, points out that he gave the "closest attention" to the content of his argument although he was at times careless about its textual arrangement. The defence of Vaihinger's hypothesis which is provided by his followers is therefore "thin," to say the least of it.

But even if we are to swallow the somewhat unpalatable and indigestible thesis that Kant included in the *Critique* passages from old manuscripts that no longer expressed his real convictions, there is at least one way of sugaring the pill. (This is the second alternative mentioned above as being more plausible

than Vaihinger's hypothesis.) We might say that Kant included these passages, not because he objected to sacrificing an outworn argument, but because he did not notice that the manuscripts in question really implied a point of view which he had discarded. Kant's language is notoriously ambiguous, and the meanings he attaches to certain terms vary from pole to pole according to their context: we can therefore perhaps imagine him reading through some old paper and mentally interpreting it in terms of his more mature teaching: or he might possibly be thought to have been betrayed by the influence of some early notes into using language which was no longer suited to express his real meaning. We might in this way explain the presence of the passages in the *Critique* which seem to reflect a crude and primitive point of view.

But even this alternative is ruled out by Vaihinger's procedure. For, if we adopted this explanation, we should at once have to urge that the passages in question must on no account be read in connexion with Kant's earlier manuscripts if we are to understand the meaning which he gave to them when he published them. The direct result of our hypothesis would be to interpret these "archaic" sections of the *Critique* in terms of what we supposed to be the maturest portions of the text. And this is just the opposite of the treatment given to them by Vaihinger and his school. Not only do they refuse to pass over certain remarks which, taken literally, conflict with other statements, and which might nevertheless have been slips of the pen, but also they attach meanings to whole passages which can only be extracted from them by reading into them allusions to such manuscripts as Kant's letter to Herz of 1772. Such procedure is quite indefensible, unless explicit reasons are given (which are not to be found in the works of Vaihinger's school) why we must in this way reject the hypothesis outlined in the previous paragraph.

IV.

And this brings us to the last line of defence for the Patchwork Hypothesis. For the test of the pudding is in the eating: and if this hypothesis enables us to present the Critical doctrine in a more intelligible form, one might conceivably accept it as merely another of the 'convenient fictions' of the inventor of the "Als Ob" philosophy.

Unfortunately, however, our hypothesis, instead of explaining the doctrine of the *Critique*, denies its existence. "It can now be proved that the *Critique* is not a unitary work, . . . but

was pieced together by the combination of manuscripts written at various dates throughout the period 1769-70" (Kemp Smith, p. xx = Vaihinger, p. 46, quoted above, p. 324). Instead of "a unified system" we have "four distinct layers" which differ "in their structure" as well as in their "age" (Kemp Smith, p. xxi and Vaihinger, p. 72). Therefore, presumably, no more time is to be wasted seeking for the non-existent 'doctrine of the *Critique*': and we must take the greatest care never to confuse the quite distinct doctrines of 1772-75, 1776-78, 1778-80, and 1780-81, which happened to get muddled up in one publication by 'an accident of paste and scissors'. In fact, we had much better not read the *Critique* at all, for there these doctrines are "modified by occasional alterations to suit the new context" (*loc. cit.*): we should do better to go direct to the manuscripts of Kant's previous years, where we can find his four 'layers' in their unadulterated form.

There seems to be no escape from this conclusion, if we are to remain loyal to Vaihinger's hypothesis. Yet Prof. Kemp Smith, who may be taken as typical of the British followers of Vaihinger, seems to consider that he is still justified in attempting, albeit tentatively, "to construct the ideal statement" to which the four layers of doctrine "severally approximate" (*Comm.*, pp. 234-235). At first this attempt would appear to be a superhuman task, seeing that, in the first of these stages, "Kant *dispenses* with the categories" (*Comm.*, p. 202), and in the other stages puts forward doctrines so inconsistent with each other that we have to ascribe them to different epochs of Kant's thought. But Prof. Kemp Smith reduces his difficulties by putting aside the first and last stages, as being "a pre-Critical or semi-Critical survival" and "hastily adopted" respectively. But, we might ask, why should doctrines of the period 1772-75 be called pre-Critical?—because they were not the doctrines held by Kant in the year that he published the *Critique*? If so, then why not treat the "last phase"—the doctrines of the year 1780-81—as the "Critical" teaching, for these were, apparently, Kant's views on the eve of publication? What virtue is there in the period 1775-80, that it alone should set the standard of "der echte Kant"? The answer is simple: Prof. Kemp Smith selects these doctrines, not because they originated in this or that year, nor because of the age of the paper on which they were written, but because he feels that they embody Kant's most valuable contributions to philosophy, because they seem to present the nearest approach in the *Critique* to a tenable and coherent system of thought, and for

other reasons of this kind. And surely he is right in doing so: for, after all, what we want to study and preserve in Kant is that part of his teaching which is logically and philosophically the most 'mature' rather than that which was the oldest or youngest in point of time.

In the last resort, then, Prof. Kemp Smith's interpretation of the *Critique* is independent of the "Patchwork" hypothesis. And, if we look into the matter, we shall see that most of his preliminary analysis turns upon logical rather than textual or historical considerations. The division of the Deduction into four groups of doctrine really depends upon the mutual inconsistency of these groups, not upon their date: the re-arrangement of the argument as a whole is due to the fact that its order in the text is far from being the truly logical order, and not because it differs from the order in which Kant actually wrote it: indeed the only part of Prof. Kemp Smith's procedure that necessitates an appeal to Vaihinger's hypothesis is the interpretation of certain obscure remarks by reference to Kant's earlier jottings—and this, as we have seen, is of the most doubtful legitimacy, even if we admit that Kant did include old manuscripts in his text. Thus, the "Patchwork" hypothesis turns out to be worthless, even as a "convenient fiction" adopted to simplify the task of interpreting the *Critique*.

V.

We can therefore safely discard the suggestion that the *Critique of Pure Reason* is a "mosaic of scraps". Its premisses are, at best, doubtful: the conclusion only follows from them if we make tacit assumptions, as to Kant's state of mind in 1780-81 and his attitude towards his public, which are quite without foundation: and, in the end, the whole elaborate theory gives us no help in grasping Kant's real meaning. But this does not mean that we are to consign the rest of Vaihinger's and his followers' work to the waste-paper basket. As we have seen, much of what Prof. Kemp Smith has to say does not depend upon its ostensible foundation. And there is, so I believe, something in Vaihinger's own suggestions that can be turned to good account.

I refer to Vaihinger's emphasis upon the history of Kant's thought between 1769 and 1781. He was mistaken, I believe, in the way he conceived the *Critique* to be related to those years. His theory implies that this relation was a purely external one—we might almost say, a mere accident of paste and scissors. And the *Critique* is not just a bundle of manuscripts: it is a

living body of doctrine. If we are to compare it with anything, we should consider it, not as a heap of rocks from different geological strata, but rather as the trunk of a tree whose 'rings' remain to record its past and yet are still living matter that combines to form an organic whole. And, as I pointed out above, I suspect that Vaihinger only substituted this mechanical relation for the organic one because he was misled by the analogy between his own hypothesis and that of Adickes. But if we avoid this error, it is undeniable that we shall learn much from the study of these early years. Certainly this study will not tell us what is valuable and what is worthless in Kant's philosophy: but if (to borrow a phrase from Adam Smith) we can "go along with" Kant by the road which led him to the Critical Philosophy, we are far more likely to see that philosophy from the inside, instead of having a superficial conception of it. And that, after all, is our first task as Kantian students and interpreters.

We are now in possession (thanks to Adickes) of a great corpus of evidence that sheds light on Kant's 'dark years' and that was quite inaccessible to Vaihinger. We now have the manuscripts of these years dated and arranged in their chronological order. Unfortunately, we have no means of corroborating the accuracy of Adickes' results, and we shall continue to have none until another scholar devotes a lifetime to his work. But we have Adickes' reputation as a careful and sober investigator: and, what is more important, we know that he has used methods and criteria (such as handwriting, and the colour of the ink), which are recognised by modern historians as scientific and reliable. We have therefore in his arrangement of the *Reflexionen* and *Lose Blätter* a clue which, if used with caution, can give us a reasonably trustworthy history of these dark years.

We are therefore able to reverse the method used by Vaihinger (and Erdmann and Reicke). Instead of dating manuscripts by the doctrine they seem to imply, we can date the doctrine by the manuscripts. No doubt it will be many years before that process can be completed: but even a preliminary survey of the material indicates that there was some degree of accuracy in Vaihinger's reconstruction of the history of these years, although perhaps it is somewhat crude and distorted.

Let us take, for example, Vaihinger's "First Phase". Prof. Paton is quite justified, in my opinion, when he contends that the passages in the *Critique*, isolated by Vaihinger as belonging to this phase, are capable of an interpretation which brings them into line with the other parts of the Deduction. But that must not be taken to mean that Kant never passed through

anything resembling this "First Phase". For there can be no doubt that Kant *did* at one time ascribe a quasi-synthetic function to empirical concepts (*cf. Reflexionen*, Nos. 3930, 3932, 3955, 3957, 3958, 3961, 3963, 3964, 3974 in vol. xvii of the Berlin edition of Kant's works). But this function was by no means a *substitute* for that of the categories, as Vaihinger seems to suggest. It would be a truer description of Kant's view (at least when he wrote the *Reflexions* just cited) to say that he regarded the empirical concepts as the pure concepts of Reason in the form in which they are applied to appearances (= the matter of sensation organised in terms of space and time), somewhat as he considered the schemata to be the applied form of the categories. As for the pure concepts themselves, I have found no evidence that he ever regarded Space and Time as the only members of this class: as early as 1769 he distinguishes what he then calls "the pure concepts of Understanding" or "pure concepts of intuition" (*i.e.*, Space and Time) from the "pure concepts of Reason" (Ground, Substance, etc.) (*cf.* Nos. 3955, 3956, 3957, 3958, etc., *loc. cit.*). At the same time Vaihinger is right in so far as he notes that Kant only refers to "pure concepts" and "rules", without calling them "categories", in the earliest years of our period. And it is only later that Kant begins to mention such things as "syntheses".

But Vaihinger would appear to be wrong in his dates. The *Reflexions* to which I have referred occur (according to Adickes) in the period of 1769-70. So this "phase", in so far as it can be isolated at all, must be placed considerably earlier than Vaihinger suspected. But it is very doubtful whether we are justified in dividing the years 1769-81 into a series of phases that are distinguished both in date and in doctrine.¹ For, it would seem, Kant's progress during these years was both gradual and com-

¹ The whole "Patchwork" Hypothesis owes its plausibility, on the other hand, to the assumption that Kant's phases of doctrine were so historically distinct that we are justified in dating a passage by its doctrine alone. How very dangerous this assumption is, can be seen from the following example. Vaihinger cites a group of eight *Reflexionen* (Erdmann, nos. 946-953) as probably belonging to the years 1772-75 ('Phase I.'). Of these, Adickes puts only one within these years, although he allows that three more may possibly have originated in this period: of the remainder he dates one as 1769-70, another as 1777-78, a third as 1776-80 or later, and the last as not earlier than 1779-80. Again, Vaihinger (*op. cit.*, p. 83) speaks of almost all the other *Reflexions* as dating from his "2nd Phase". Adickes, on the other hand, puts about 1000 jottings as prior to this date (including, however, a number of *Loose Blätter* and hitherto unpublished *Reflexions*), and only a few hundreds in Vaihinger's remaining three "phases" put together.

plicated. First one problem would be grappled with : and then another would become ' the question of the moment '. One line of thought would be followed for some distance, and then neglected while Kant went off on a new tack : and the conclusions reached from the first investigation would be left in the background without being positively rejected. Sometimes we find that Kant returns to his first problem and revises his opinions there in terms of the result of his second inquiry : but sometimes the earlier reasoning would be " left on the shelf " without revision (especially if Kant had been satisfied with it in the first instance) while he busily adds to his store from other quarters. . . . And so, when Kant came finally to take stock of his many-sided investigations, there would probably be brought to the forefront not only his latest conclusions upon some recent topic, nor even those doctrines only which he had revised from time to time, but also trains of thought that had lain at the back of his mind without being reconsidered, and had become all the more plausible to him because he had held them unquestioningly for so many years.

IV.—DISCUSSION.

A CONTRADICTION IN COMMONSENSE ETHICS.

THE principle that 'I ought always to be guided by the dictates of my Conscience' is recognised by commonsense to be a fundamental and unarguable maxim of moral conduct. Most philosophical writers about ethics have overtly or implicitly made use of the same principle, including many who have held that there are no other self-evident maxims in ethics. The only sort of ethical theory which is necessarily inconsistent with its truth is Psychological Egoism. But its implications for ethical theory have, I believe, never been satisfactorily worked out. The meaning of the principle is not generally in dispute. All normally constituted moral beings have an impulse to do what seems to them to be right. They have also many other impulses which may, and often do, conflict with this one. Whenever such a conflict occurs they ought to be motivated by the impulse to do what is right and not by another.¹

On the other hand, it is admitted that men are often mistaken, even after earnest and sincere deliberation, in their beliefs about what is right for them to do. Hence they may believe that conduct is right which in fact is wrong. But the principle enunciated states that it is always right for me to do what seems to me upon adequate reflection to be right. It follows that cases will arise in which it is right for me to do what is wrong. But that is contradictory. For it cannot be both right and wrong for me to perform the same action. This contradiction arising out of the conjunction of an intuitive ethical principle and a recognised matter of fact (that ethical beliefs may be false) forms the basis of this paper. The contradiction certainly prevails at the commonsense level of ethical theory. Has it been superseded at the philosophical level?

The question naturally arises whether the apparent contradiction derives from an ambiguity in the meaning or application of 'ought,' and would disappear upon further analysis. Were this the case, the implicit acknowledgement of the principle, together with an implicit recognition that Conscience may err, should not lead to practical difficulties. For a non-existent theoretical contradiction cannot bear actual fruit. But such practical difficulties are frequent

¹ 'Conscience' is used throughout this essay of that faculty by which we are aware of ethical principles or ethical facts, without further implications as to its specific psychological nature.

and necessitate practical compromises at the expense of logical consistency. An example would be a religious fanatic who thought it right to act in such a way as to cause suffering or death to many worthy people who disagreed with him in their beliefs about the Deity. Commonsense would regard such an action as wrong. But if it were conscientious, and especially if it were performed despite impulses of pity and humanity, commonsense would be in a dilemma. So arises the notion of the worthy though mistaken man.

But such practical considerations cannot prove the theoretical point. For practical difficulties might arise from other sources than latent incompatibility of theoretical principles. It will be necessary to inspect the notion of 'ought' with a view to discovering whether it contains any hidden ambiguity which may be relevant to the issue raised. I have followed the admirably lucid analysis given by Dr. Broad in 'Five Types of Ethical Theory' (pp. 161 ff.).

'Ought' is used with a wider and a narrower application. In its wider application, Dr. Broad suggests that it is reducible to the notion of the Ethically Right or Fitting, "together with the associated notion that, if the right state of affairs were in the power of anyone to produce, he ought to produce it." In its narrower application 'ought' is limited to the class of things which are in the agent's power to produce by volitional activity. I think it follows from this, although the implication is not explicitly drawn by Dr. Broad, that, if I could not do what is most right under the circumstances, although I should will to do it (*e.g.*, love my enemy), I ought nevertheless to choose the course of action which is most right of those which *are* within my power. The most right action which I could do if I willed, is a perfectly determinate notion, and I shall call such an action the *best practicable action*. (The best practicable action is analogous to G. E. Moore's "best possible action," the latter notion being derived from a teleological conception of Right). It seems to me that the idea of 'ought' as hitherto described implies that I ought always to perform the best practicable action.

But the best practicable action is not necessarily identical with that which I believe to be the best practicable action. For I may be mistaken about what I could do if I willed, and I may be mistaken about what is most right or ethically fitting in the circumstances, or, finally, I may be mistaken about the nature of the circumstances in which I am called upon to act and about the probable consequences of my action. Hence the principle that I ought always to perform the best practicable act seems to conflict with the principle that I ought always to obey my conscience. For I cannot be obliged to two incompatible courses of action unless they are equally right and the choice between them ethically indifferent. Dr. Broad coordinates the narrower application of 'ought' with the latter principle, for he says that it is bound up with the facts that the belief that an action is right is a motive for doing it, and that when this

motive is in conflict with other motives it should always win (p. 166). Yet this motive has admittedly often led men to do actions which are wrong. It seems equally true that I ought always to do the best practicable act, and that I ought always to do what I believe to be the best practicable act. The two are not necessarily identical, and if on any occasion they are identical that is a purely contingent fact. Either we are dealing with two different and opposed meanings of 'ought,' having the same application (to those things which the agent could do if he willed), or we seem to be faced with two fundamental and contradictory principles in ethics. But 'ought' is not in either case envisaged in a purely logical sense, and the contradiction is not affected whether we assign to it the deontological or the teleological meaning. At the least, then, we may claim that the contradiction noted in commonsense ethics does not automatically disappear in philosophical ethics. We have found it to depend upon two inconsistent *principles*.

Several modes of solution naturally suggest themselves.

1. It might be maintained that the proposition 'I ought always to obey my Conscience' implies that any action the motive of whose performance was the agent's belief that it was the best practicable action, was in fact the best practicable action. And the following argument might be advanced in defence of the seeming paradox. Any state of affairs which contains as a constituent part the voluntary action of a moral being has, as a whole, the ethical characteristic of being Right or Wrong (good or bad). If, as is often the case, the motive of the agent is the belief that the proposed action is right, or some other motive in conflict with this motive, that fact will be an element in the total state of affairs. And it will itself be intrinsically right or wrong. In all such cases, it might be argued, action from, or in opposition to, Conscience, has this unique characteristic, that it determines the ethical character of the total situation of which it is a constituent part. If this were the case I should always be performing the best practicable act when I am guided by my belief that any action is right, even though that belief were mistaken.

But the postulate on which this contention relies is in opposition to ethical judgments whose truth it would be fantastical to deny. The action of the religious fanatic who endeavours to cause the death of such people as disagree with his views about the nature of God is wrong, although it may well be conscientious. And even though the extreme view were held that the only examples of ethical Rightness are actions of moral beings which are conscientious, it would be possible that certain actions of this kind should tend to lessen the total amount of Rightness in the universe. For they might be 'stumbling-blocks' to others, making it less easy for them to act conscientiously, and hence less probable that they would act conscientiously. But it seems obvious that it is my duty to act in such a way as to increase the total amount of rightness in the universe

so far as is in my power. And to act in such a way would be to perform the best practicable action. Hence the opposition still remains between the duty to obey Conscience and the duty to perform the best practicable action.

Moreover, if in all ethical situations the ethical value of the motive is alone determinative of the total value of the whole situation, the assertion that one man has more accurate or more obtuse ethical perception than another becomes nugatory. And the duty to endeavour to improve my perception of right and wrong disappears. For I cannot by so doing increase my power to produce ethically valuable situations. Rather, if it is true that it is more arduous to live up to a finely discriminating than to an obtuse sense of right and wrong, the possession of such a sense would render it less likely that I should produce ethically right situations. This contradicts the universal intuitive notions of duty of mankind. And it is illegitimate to supersede one self-evident principle by another, unless the one is seen to be derivative from or contained in the other.

2. It is obvious that if on every occasion I performed the best practicable act I should produce the greatest amount of positive ethical value during my life that I was capable of producing. It is equally obvious that the chances are infinitesimal that by obeying his Conscience any human being will always do the best practicable act. And the chances that he would do so by any other rule of life would seem to be less. But it would seem to be self-evident that any man ought to endeavour to produce a greater amount of ethical value throughout his life, rather than a less amount. And most men would be likely to produce a greater amount by consistently obeying Conscience than by any other rule of life or by adopting no rule. Hence if I ought to produce a greater rather than a smaller amount of ethical value on the whole, I ought consistently to obey my Conscience. The second 'ought' is logical. The principle that I ought always to obey my Conscience is a practical rule derived from the ethical and ultimate principle that I ought to produce a greater rather than a smaller amount of positive ethical value on the whole. I do not think that this is opposed to the intuitive ethical convictions of commonsense, but that commonsense does not as a rule succeed in going behind the principle of obedience to Conscience which it takes as ultimate instead of derived. But so long as each ethical situation is taken independently it is impossible to go behind that principle. It is necessary to take the whole life of a moral being as our unit.

The principle 'I ought always to do the best practicable act' will, on this view, no longer be a concrete ethical principle involving moral obligation, but an ethical ideal or extrapolation. It simply states that if anyone always performed the best practicable act he would cause a greater amount of positive ethical value throughout his life than under any other circumstances whatever. It is analytical and not synthetic. Moreover, it is contradictory to assert that

anyone ought *always* to do the best practicable act, for although the definition of 'best practicable act' involves that on any particular occasion the agent could perform that act if he willed to do so, we have seen that he could not if he willed render it even likely that he would *always* do the best practicable act. I do not think this conclusion is opposed to anything in commonsense or practical ethical notions. We should rather notice a tendency among philosophers to take an abstract ideal too concretely.

But the initial conflict is not yet completely solved. It is probable that for most men the rule of obedience to Conscience would ensure the greatest amount of positive ethical value on the whole, but in the case of some men a more complicated principle might seem necessary. It is not clear that both the religious fanatic and the humanitarian, both a Nietzsche and a Soloviev, would produce the greatest amount of positive ethical value by following each his own Conscience. In the case of the religious enthusiast, for example, one might suggest that he would produce more value in his life if he made a rule of following Conscience except when it conflicted with the emotions of pity or humanity. No particular rule of this kind could of course be justified. For it would involve setting the ethical convictions of one man, or group of men, above those of another. And there is no more ultimate authority than Conscience itself or ethical conviction, by which such a procedure could be justified. But the suggestion has also theoretical difficulties. For commonsense claims to recognise in the maxim 'I ought always to obey my Conscience' an *universal* principle. The whole state of affairs involved in the suggested action of the religious fanatic would be generally judged to be disvaluable. But the fact that his action was in obedience to Conscience (which is an element in the whole state of affairs) has positive value. Its rightness is not negated by the disvalue of the whole of which it is a part. Such judgements occur most frequently when a man knows that his belief about what is right is opposed to the beliefs of most men of his age or country, and follows his own conviction at the expense of odium or unpopularity.

Nor does it help to assert that, when I judge that a certain person was right in that he obeyed his Conscience but wrong in that his Conscience was mistaken, I am making two independent judgements about different objects, the ethical characteristics of his motive and the ethical characteristics of his behaviour. The original dilemma is thereby simply restated. For he was right to do what he believed to be right but what he did was wrong. And our object was to examine the seeming paradox that it is sometimes true that I ought to do what is wrong: since, unless some more ultimate principle can be found, '*x* is wrong' implies 'I ought not to do *x*'; while the principle that I ought to do what I believe to be right may involve that I ought to do what is really wrong.

3. We must therefore once more endeavour to widen our ethical

vision. In place of the principle 'I ought to adopt that rule of conduct which is likely to ensure that I shall produce a greater amount of positive ethical value throughout my life than would be ensured by any alternative rule,' I would suggest the larger principle 'I ought to adopt that rule of conduct which is likely to ensure that all men present and to come will produce a greater amount of positive ethical value throughout their lives than would be ensured by any alternative rule'. The rule of obedience to Conscience was seen not to be indicated by the former principle in all cases, although it was indicated in the great majority of cases, and it was found impossible to decide in particular cases that it was or was not indicated. But it would seem to be indicated in all cases by the wider principle. For, as human nature is constituted, any relaxation of this rule would have far-reaching effects, and would tend to cause it to be neglected in cases where it ought to be adopted. And the impracticability of deciding when it should be supplemented and when strictly followed tends to the same conclusion. For the only alternative to its universal adoption is the majority vote. And that would suppress ethical geniuses as well as ethical degenerates. The argument of course presupposes that Conscience is on the whole a veridical faculty, and that general changes in ethical beliefs are on the whole towards greater truth rather than towards greater falsity. But this assumption must be common to all serious ethical theories.

If we are right, the paradox cannot be solved by any theory of ethics which takes the particular ethical situation or the single individual as its unit for discussion. It is necessary to take as the ultimate unit for ethics the notion of mankind as a single ethical substance, composed of separate ethical subjects standing in ethical relations to each other and to the universe, and ethically progressive. The conception of reality which is more favourable to this point of view than any other of those commonly held by philosophers is that of Ethical Theism. And in addition it would, if it could be advocated without having recourse to ethical postulates, lend a high degree of probability to the assumption that Conscience is on the whole veridical, that mankind tends to progress in the discovery of ethical truth, and that the universal adoption of the principle of obedience to Conscience would tend to cause a greater amount of ethical value in the universe than would be caused by the adoption of any other practical principle whether universally or by a small minority of individuals. But it is almost certain that Ethical Theism cannot be rendered probable without having recourse to one or more of these postulates.

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V.—CRITICAL NOTICES.

Five Types of Ethical Theory. By C. D. BROAD. London: Kegan Paul, Trench, Trubner & Co., Ltd., 1930. Pp. xxv + 228. 15s.

A BOOK on ethics by Dr. Broad is bound to be welcome to all lovers of clear thought. There is no branch of philosophical study which stands more in need of the special gifts which mark all Mr. Broad's writing, great analytical acumen, eminent lucidity of thought and statement, serene detachment from irrelevant prejudices. Indeed, one might even say that the very defects inseparable from these qualities contribute to the peculiar merit of the book. One such defect is confessed in the apology made in the *Preface* (p. xxiv.) for want of "experience, both practical and emotional," and the lack of what other men call a vivid "sense of sin". I do not doubt that these are real defects in a moralist. If he really has *no* experience—though Dr. Broad's language about himself is presumably not to be taken too literally—of serious struggle with temptation to grave wrongdoing, and knows nothing of keen emotional penitence for wrong done, even a careful and acute analyst of the facts of the moral life is likely to see too much only what lies fairly on the surface. In morals, as in art, there is a childlike innocence which does disqualify its possessor from plumbing the depths. A man need not have been personally a gross sinner before he can be a profound moralist, but perhaps he does need to be able in imagination to "go along" with the sinner, as Plato or Augustine could, and as Sidgwick, or even St. Thomas, pretty plainly could not. And a man is not very likely to understand a subject to the bottom if the contemplation of it cannot stir him to a white heat of emotion. Even the mathematician, I take it, must be 'in love with' geometry, to be quite in the front rank of geometers. But on the other side, it may be said that the heat of the passionately ardent soul is more often a *red* heat than a white; its vision may pierce deep, but is apt to be turbid, whereas whatever Dr. Broad sees, he always sees with singular clarity, and he can tell us very lucidly just what it is, as one would expect of a Fellow of Trinity and a devoted admirer of Trinity's famous moralist, Henry Sidgwick. I should not myself complain, as some readers perhaps may, that the author's very detachment leads to an entire freedom from metaphysical prepossessions of every kind. No doubt, morality, like everything else, has its metaphysical presuppositions, and the last word has not been

said about it until it has been ascertained what they are. But before we can profitably ask that question, it is surely necessary that there should be a thoroughly worked-out account of the "phenomenology" of the moral consciousness to go upon, and Dr. Broad's discussion of his five typical moralists seems to me an exceptionally valuable contribution to such a phenomenology.

There are, in fact, only two minor peculiarities of manner and temper on which, if I may be forgiven, I should like to utter a word of friendly remonstrance. One is a tendency to a rather forced and self-conscious facetiousness, which shows itself not for the first time in the present volume. Is it permitted to hint that the charm of humour lies very much in its spontaneity? As soon as it is consciously sought after, it becomes facetiousness, and the facetiousness of even such a natural humorist as Dickens is annoying to a really cultivated taste. (*À propos* of Dickens, I could wish that on page 31 there had not been an unhappy confusion of Mrs. Jellyby with the landlady of the *Marquis of Granby* inn.) The other is that Dr. Broad occasionally allows himself really irrelevant expressions of disputable personal opinion, apparently, like Alexander Pope, in the amiable desire "to vex somebody." It may be that T. H. Green was a "second-rate" thinker, and again that Sidgwick was a greater philosopher than F. H. Bradley, but these dicta are manifestly capable of being disputed, are not really pertinent to the argument, and may be felt by the members of other societies to be little more than an expression of the natural partialities of a Cambridge man and a Fellow of Trinity.

Mr. Broad's five typical moralists are Spinoza, Butler, Hume, Kant and Sidgwick. The chapters on the first four, originally delivered as public lectures (that on Butler in his own cathedral city of Bristol, the others in Dublin), are comparatively brief but admirably penetrating statements of the main tenets of each thinker, accompanied by acute criticisms intended for a highly intelligent but not specialist audience. The treatment of Sidgwick, which is on a much larger scale, has been specially added to complete the volume, and bears marks of having originated in the systematic study of the *Methods of Ethics* with pupils reading for the Moral Sciences Tripos. A brief biographical account of the lives and characters of the five moralists is prefixed, and the book ends with an only too brief statement of the writer's own convictions on some of the fundamental issues of ethics. I propose to offer a few remarks on each of these divisions.

The short biographical introduction naturally gives occasion for no very special comment. Perhaps I may just say that it seems to me very difficult not to believe that Spinoza's visit to the French Headquarters in 1673 was really connected with some negotiation which could not well have been undertaken openly, so that the popular excitement about it at The Hague may not have been so wholly unreasonable as Dr. Broad seems to suppose. Also I think it only fair to Dr. Johnson to say that what gave him "offence" in

1776 was not so much Hume's own composure in the face of death as the certainly rather indiscreet published language of Adam Smith about Hume's last days. And I doubt whether Kant's *Metaphysik der Sitten* should be described as a "second part" of the *Grundlegung*.

Far the hardest task Dr. Broad has set himself is that of giving an intelligent audience, with no previous acquaintance with the subject, an account of Spinoza's doctrine which shall be at once brief, clearly intelligible, and full and accurate enough to serve as the basis for really effective criticism. Hard as the task is—and it involves explaining not only the Spinozistic view about mind and body, but the doctrine of the *idea ideæ*—Dr. Broad has succeeded to a marvel; I doubt whether any other existing statement of this point of view is likely to be found so clear and helpful by the student who is beginning to face the difficulties of the author for the first time. In sketching the moral theory of the *Ethica*, Dr. Broad, naturally enough in view of the conditions, confines himself to the *Fourth Part* and the first half of the *Fifth*, omitting everything connected with the conceptions of "knowledge of the highest type" and the "intellectual love of God." This was probably unavoidable, and is certainly permissible, in view of the avowed intention to discuss a certain type of moral theory on its own merits, apart from the historical question how far the theory really represents the views of a particular philosopher. But, as Dr. Broad is well aware, the consequence is, of course, that what has to be left out is just all that really matters most in the *Ethica*, the doctrines which meant most to Spinoza and have always done most to make him attractive to the finest type of mind. I am glad to have Dr. Broad's recorded support for the conviction that these doctrines are entirely inconsistent with the "naturalism" of the main bulk of the *Ethica*, so lucidly expounded in Dr. Broad's lecture; the inconsistency, if meditated on, raises a very important question which it would not have been to Dr. Broad's purpose to discuss, the question whether it is not, in the end, impossible to sever ethics from religion without destroying the distinctive character of ethics itself. In the actual exposition of *Ethica*, II.-IV., I presume Dr. Broad is fully alive to the fact that the really terrible difficulties of Spinoza's doctrine of mind and body are not removed, but merely decently veiled, by the usual phraseology about the heterogeneous but inseparable twin "aspects" of all events. The further, and to my mind, even more perplexing doctrine of the relation between the *idea* and the *idea ideæ* is probably put in as favourable a light as it can receive by Dr. Broad's interesting illustration of the patient suffering from indigestion and his physician; but here too, I take it, Dr. Broad would not maintain that the difficulties are really more than disguised.

For my own part, I cannot succeed in making the theory, that the "object" of every idea is always a state of the body with which the mind to which the idea belongs is connected, intelligible. When

I know that $e^m = -1$, or that Cæsar crossed the Rubicon, I cannot see that in knowing these things I am being "acquainted" with any state of my body, though Dr. Broad is clearly right in saying that Spinoza believes this and thinks it a truth of tremendous importance. To me it seems simply false, and I cannot escape believing that Spinoza is merely equivocating about the meaning of "object". The equivocation seems to me a complicated one, and I suspect that it originates in some such way as this. An *object* is thought of as a representation which "mirrors" something represented and is *caused by* that which it "mirrors" (the traditional Neo-Platonist way of thinking about causality). The representation is then supposed, very inconsistently, at one and the same time to be caused by the thing represented, and also to be caused by the state of my "cerebral centres" apart from which, it is assumed, there would be no representation, and these two points of view are thrown together by the ambiguous use of the word 'object' to mean at once that of which I am aware in knowledge, and also a process in my body assumed to be a condition of my having the knowledge. Finally, it is forgotten that the whole theory originates in a certain view about the nature of causality, and it is actually denied that either Cæsar's passage of the Rubicon or the state of my brain has any *causal* connexion with my knowledge that Cæsar crossed the river. I may be exposing my own incompetence in this confession that I can see nothing but complicated confusion in what has been regarded as the profoundest of philosophical truths, but I really see nothing better in it. On the further problem about the *idea* *ideæ* Dr. Broad says, so far as I can see, all that man can say, but I am sure he knows that he is merely evading the question which he admits that Spinoza's doctrine raises, "*where* are the adequate ideas corresponding to the vital processes in the life of an animal's body?" I do not myself see how Spinoza could have answered the question without constructing a fantastic mythology, and I think the correspondence with Tschirnhaus indicates that he came at last to see that there is an unsolved problem here.

On the more specially ethical issues involved in his discussion I think Dr. Broad generally admirable; in particular, I would call attention to his, as I think it, unanswerable criticism of the famous denial that *consciousness* of my desires affects my action, and to his instructive comment on Spinoza's ingenious attempt to deduce a rule of social beneficence from strictly egoistic premises, that all that has been shown is that an egoistic "free man," *living in a society of persons who are not "free" enlightened egoists*, would have good reason for general conformity to *their* moral code. There are just two further points on which I could wish that Dr. Broad had said something more than he has done. Is there any reason to believe that the practice of "considering all that befalls us as inevitable" would really lead, as Spinoza assumes, to a true peace of mind? It seems to me that it would be more likely to result in a

dreary *acedia*, unless it was accompanied, as was the case with the Stoics, by a religious confidence in the "working together for good" of all events; and this is logically excluded by Spinoza's hostility to teleology. And, again, had Spinoza any right to retain any distinction between good and bad in his system? It is true, as Dr. Broad says, that he reduces the distinction to the level of 'naturalism' by making good mean "appropriate to the characteristic functions of the species". But is the implied belief in the reality of the species consistent with Spinoza's more nominalistic utterances? The correspondence with Blyenbergh shows what difficulties Spinoza had on the point when closely pressed.

The account of Butler, a favourite thinker with Dr. Broad, seems to me almost as good as could be given, and I am particularly glad to find Dr. Broad giving no countenance to the attempt made, on absurdly weak grounds, to show that Butler ever co-ordinated "self-love" with "conscience". If I may suggest a mild criticism where I find myself in almost complete agreement, I would urge reconsideration on one or two points. I do not see why Butler's 'teleology' should be deprecated on page 58. Dr. Broad says that Butler's language is only justified on the assumption that "man has been made by God" for a certain purpose. I should say rather that the familiar theological phrase is a pictorial way, and a very good pictorial way, of stating something which is *obviously* true, viz., that there is a real teleological unity pervading the whole world, as another eminent Cambridge philosopher is just now asserting very vigorously. The existence of God seems to me to *follow* naturally from this all-pervading teleology, but I do not see that Butler's argument necessitates more than the recognition of the fact. Again, I doubt whether Butler, who always speaks in the *Sermons* of "three" active principles, would altogether have acquiesced in the addition of 'benevolence' as a fourth (p. 60). In the *Sermons*, I think, Butler probably regards the systematic practice of universal benevolence as a case of that "joining" of "conscience" to a "particular passion" of which he speaks in the *Preface*. I do not see that he requires a fourth "principle" to account for it, any more than he needs a fifth to explain the behaviour of a man who prosecutes for years a systematic scheme of revenge on his opponents, or a sixth to deal with the case of the man who devotes his life to making a fortune on the Stock Exchange. In the *Dissertation*, I even feel that "interest" or "self-love" itself is reduced to a lower rank than that given it in the *Sermons* by the suggestion that neglect of our own happiness is wrong, not so much because it is opposed to 'self-love,' as because "conscience" forbids it. Butler seems to me here on the straight way to the view that *all* "imperatives" are deliveries of "conscience". Also it is surely by an oversight that on page 81 the suggestion that "God is a Utilitarian," is spoken of as though it represented Butler's own tentative opinion. Read along with its context, the suggestion must surely be taken as one which

Butler regards as impiously presumptuous to make. His point is only that *even on that hypothesis* we should have no excuse for attempting to promote universal happiness in ways which "conscience" condemns.

I may deal more briefly with the luminous account of Hume. In view of much that is commonly said about Hume's view of good, I think it an important point, with Dr. Broad, to distinguish various types of Hedonism, and to insist that the Hedonism of Hume is *synthetic* (i.e., that he does not regard "pleasant" as the whole, or part, of the *meaning* of "good"), and *empirical* (i.e., that he takes it as a mere inexplicable case of "conjunction" that whatever is good happens also to be directly or indirectly pleasant), and this is why, to make his theory work, he has to assume the reality of the "feeling of humanity," as well as the reality of the tendency to "sympathy". I particularly commend the force with which Dr. Broad states the insuperable difficulty of accounting for the approbation of Justice on Hume's principles. I, like him, feel sure that (p. 97) we should *not* unreservedly approve a distribution of property which we judged to be markedly unfair, even if we knew that it stimulated production so efficiently that every one got more happiness than would result from a fairer distribution. It does seem to me certain that disapprobation of unfairness is as fundamental a characteristic of human nature as dislike of pain, and that unfairness is resented simply because it is unfair. This, I think, is the strongest *obvious* point in the case of the Rationalist in ethics against the Sentimentalist. It is worth noting that even Bentham had to introduce an independent principle of Justice into his scheme, to get it to work. (For he never attempts to show that the rule "every one to count for one" has any logical connexion with the "greatest happiness principle".) A particularly good feature of the discussion of Hume is the account of the functions of "Reason". These, according to Dr. Broad are three: (1) the function of *a priori* concepts, (2) inference, (3) the perception of necessary connexion between characteristics. Hume always ignores (1), and in his ethical works, though not always elsewhere, he ignores (3) also. Hence the futility of his professed demonstration that Reason plays no part in determining our approbations and disapprobations.

In the generally excellent account of Kant, there are, I think, one or two awkward slips. It is surely not true that Kant either held, or tried to prove, that nothing is *intrinsically* good but a good will. What Kant says is something quite different, that nothing is *always* and *unconditionally* good but a good will. ("Happiness" is not; for, according to Kant, happiness is only good when it is the consequence of virtue: but it is essential to Kant's moral theology that the happiness which comes to the virtuous as a consequence of their virtue *is* intrinsically good. If it were not, the moral problem to which Kant regards Theism as giving a solution would not exist.) Again, it is not a just criticism to say (p. 117) that Kant has only

produced evidence to prove that "a good will is a *necessary constituent* of a whole which is intrinsically good," and that this leaves it possible that the good will by itself should have *no* intrinsic worth. It is precisely to exclude any such view that Kant appeals to what he takes to be the admitted fact that we judge that a good will prevented by circumstances from taking effect loses none of its value, but "shines like a jewel in the dark". Dr. Broad may hold that this judgment is mistaken, but he must surely admit that if it is, as Kant thought, a true judgment, it proves the point at issue.

Again, I doubt very much the accuracy of such a statement as that on page 118 that Kant holds that an action "cannot be right" unless it is done on some general principle which the agent accepts. I do not think we can fairly make the equation "acts which have moral value" = right acts. It has always seemed to me that Kant meant that such acts, and only they, are *meritorious*. He cannot have supposed that it was "not right" in himself to smoke his morning pipe, because he was not doing so for the sake of obeying a Categorical Imperative. I think he only meant, to put it crudely, that one does not get a "good mark," any more than a bad one, for acts which are in accord with duty, though not done "from duty." At least, I am sure that we ought to consider seriously whether this was not his meaning; and, to prove that it was not, we need to show that he actually thought it *wrong* to do anything on impulse. On the other side, when Dr. Broad absolves Kant from the "foolish" accusation of regarding the individual conscience as infallible (p. 122), he seems to have forgotten Kant's express assertion that an "erring conscience" is an *Unding*. I cannot here follow Dr. Broad through the course of his acute criticism of the Kantian Ethics, and must content myself with the remark that while, as I think Dr. Broad would readily admit, the defects noted in Kant are in the main those which have given offence to earlier critics, the singularly lucid and delightfully untechnical exposure of them is altogether Dr. Broad's own. Space only permits of my adding that I do not feel satisfied by the attempt to discriminate between two or more senses of the word "ought" which is employed at page 141 ff., and again, with still more subtlety at page 163 ff. (in the chapter on Sidgwick) for the purpose, partly, of invalidating the foundations of a "moral theology" (in Kant's sense of that phrase). I should certainly agree with Dr. Broad that there is a certain difference in meaning between the "ought" of "I ought to do this," and the "ought" of "This ought not to be", but I doubt very much whether it is true to say that the first of these "oughts" involves "factual," the second *only* "logical" possibility. I must not attempt to argue the point here, and will merely remark that if the second "ought" only means, as Dr. Broad says, that the state of things called "this" involves no logical contradiction, and "that any being who could bring it about ought to try to do so," Dr. Broad's case would be made out. But my own feeling is that the

second "ought" involves a good deal more than this, though it may be hard to say just how much more, exactly as Dr. Broad himself holds (p. 195) that the doctrine of Free Will contains an important truth, though he professes himself unable to say just what that truth is. When a man says with real conviction that "virtue ought to be rewarded," he does not, I feel sure, mean no more than that "if there is a good God, He will reward virtue," nor yet, I think, is it meant simply to *assume* that "there is a good God". What is assumed, I believe, is something not easy to formulate exactly which goes beyond the first of these positions without being equivalent to the second.

I have said so much about the first half of Dr. Broad's book that I am compelled to abstain from commenting at any length on the very interesting second half in which he gives us a very careful analysis and criticism of Sidgwick's *Methods of Ethics*, followed by a few pages indicative of personal convictions on ethical questions. I am the more sorry to be forced to do this that I find the questions raised by the analysis of Sidgwick the most interesting part of the whole work. But I do not see how the views Dr. Broad develops in these analyses could be adequately discussed except in a whole series of essays. It is particularly interesting to know that Dr. Broad can be fairly claimed as a Rationalist in Ethics, in the more reasonable sense of the epithet, on the strength of his conviction that the notion of *right* is an *a priori* concept, and again, that (p. 194) he agrees with some of the rest of us in holding that 'determinism' is really not respectable as a theory of voluntary action. (At least he wittily compares the "scientific explanations" of the determinist (p. 194) with those of Molière's doctors.) I hope I may be pardoned if I suggest that, in the interesting attempt to make a classification of possible moral theories, too much, perhaps, is made of a distinction between "naturalistic" and "non-naturalistic" theories of morals which, as stated, is really ambiguous. I do not doubt that with a sufficiently rigid definition of "naturalism" the distinction is of great importance; my difficulty is that Dr. Broad's own definition seems to me not careful enough. A "naturalistic theory" is said (p. 257) to be one which holds "that ethical characteristics can be analysed without remainder into non-ethical ones," and so far, no doubt, the formal definition is one which is beyond criticism. But on page 259 we find it further said that it is "naturalism" to define virtue as the efficient performance of the "activities of the species to which you belong." Now, on the face of it, it should follow that not only Spinoza, but Aristotle, and even Plato, who certainly would not have denied that "doing the specific work of man" is what we mean by being virtuous, are "naturalists". And an interpretation of the word "naturalism" which will make it extend to Plato will hardly allow us to refuse the name "naturalist" to *any* moral philosopher. I think, therefore, that the real distinction between "naturalistic" and "non-naturalistic" theories of morals

has somehow slipped through Dr. Broad's fingers in the course of his discussion. Is it not an intelligible position to hold that virtue may be quite correctly defined as "efficient discharge of the functions of our species," and also to hold that when we come to say what these functions specific to "man as man" are, we require to introduce the ultimate and indefinable notions of "good," or of "right," or possibly of both? What makes Spinoza, at any rate in all but the concluding section of his chief work, a "naturalist," is surely not that he regards virtue as "living the human life efficiently," but that he thinks he can say what the *human* life is adequately in the language of biology?

One other word by way of defence of certain memories dear to Oxford men. I think that devotion to Sidgwick has led Dr. Broad all through his book to attach too much importance to the opposition of Egoism and "Altruism," an antithesis which seems to me after all a secondary one, both in moral theory and in moral practice. A consequence of the stress laid on this antithesis is that both Green and Bradley figure as 'egoists,' and, in view of the meaning of the word 'egoist' taken over by Dr. Broad from Sidgwick, I submit that this amounts to a perversion of the thought of the two Oxford philosophers. Nothing was further from the teaching of either than "egoism" as defined by Sidgwick, the view that the only good it is incumbent on me to pursue for its own sake is good to be personally enjoyed by myself. It is true that both Green and Bradley held that my business as a moral agent is to become a genuine moral *person*, but they also held, as Bradley explicitly put it, that it is only by becoming a member of a "larger whole" and finding my own good in the promotion of the good of that "whole," for its own sake, that I can really grow into moral personality. The whole point of Bradley's exaltation of "my station" is that, in setting myself to discharge the "duties of my station" efficiently, I have a definite scheme of action marked out for me, in respect to which it is strictly irrelevant to ask whether the "results" it secures are to be enjoyed by myself, or by my neighbour. To be sure anyone who decides to call a doctrine of this kind "egoism," on the ground that Green and Bradley insist so much on the point that the whole process is the process by which the agent becomes a true moral *ego*, is strictly within his rights. But a real injustice is committed when the egoist in this sense of the word is identified with Sidgwick's "rational egoist," or is supposed to be maintaining psychological theses refuted in advance by Hume or Hutcheson.

I must once more apologise for the inadequacy of my treatment of so fascinating a book as Dr. Broad's.

A. E. TAYLOR.

The Morality of Punishment. By A. C. EWING. London: Kegan Paul, Trench, Trubner & Co., Ltd., 1929. Pp. xiv + 233.

THIS book is based on a thesis which gained for the author the Green Prize in Moral Philosophy at the University of Oxford. It is introduced by a short foreword from the pen of Dr. W. D. Ross, and it seems to me to be a most able and interesting contribution to Ethics.

In the Introduction and in Chapter VI., on "The Bearing of Our Moral Theory on Practice," Dr. Ewing states and defends his general view of Ethics; in the remaining chapters he deals with the ethics of punishment and reward. Much of what the author says about the special problem is independent of his view on the general nature of ethical cognition. But the latter is of considerable interest and importance, and readers of the book would be well advised to study the two chapters which contain it before they embark on the more special enquiry from which the essay takes its title. I propose to follow this course in the present review.

The Introduction states the general propositions about ethics which will be assumed in the discussion of the morality of reward and punishment. It is assumed that ethics is concerned, among other things, with the goodness or badness both of acts themselves and of their consequences. It is further assumed that ethical hedonism is false. Pleasure is good and pain is evil, but pleasantness and painfulness are not the only factors which are relevant to the intrinsic value of a total state of affairs. Next, it is assumed that there is well-grounded belief, if not genuine knowledge, of what is good and right in certain cases. Not all moral judgments can be proved, and none can be proved from wholly non-ethical premises. There must then be some ethical intuitions, though we need not suppose that any of them are infallible. But reflexion and reasoning are also needed, especially to analyse complex situations and to distinguish and estimate the various good and bad features in them. Ethical judgments, like all other judgments, are true or false independently of the opinions, desires, and emotions of those who make them; and those who deny this are deceived by certain confusions of thought. No doubt, *e.g.*, the right action for A may differ from the right action for B in the same situation S. But, when this is so, the difference will depend on some assignable qualitative difference between A and B. Again, it may happen that the agent is the only person who is in a position to know what is the right action for him to do in a certain situation. But this does not affect the objectivity of right and wrong. There is of course much apparent diversity of opinion about moral questions. But most of it is *only* apparent. And, even where it is real, it does not prove that there is no objective truth or falsity about right and wrong, any more than the disagreements between historians about the motives of statesmen prove that there is no objective truth or falsity in history. There is, indeed, a sense in which it is always right to

do what one honestly believes to be right, and wrong to do what one honestly believes to be wrong. Nevertheless, a conscientious action may be objectively wrong even for that agent in that situation, though the agent must be blamed if he acts against his conscience and cannot be blamed if he acts in accordance with it. Lastly, it is true that the subject of a moral judgment is always "subjective," in the sense that ethical predicates apply only to minds, their experiences or dispositions, and wholes in which these are essential constituents. But this does not make moral judgments themselves "subjective," in the sense of expressing merely the personal feelings or opinions of those who make them. All this seems to me to be plainly true, and to be put with admirable clearness.

The problem which Dr. Ewing discusses in Chapter VI. is primarily epistemological. How do we reach knowledge or rational belief about the right course of action in a given concrete situation? He begins by trying to refute a certain answer to this question, and then he goes on to develop his own solution. The theory which he tries to overthrow is that such knowledge or rational belief could in all cases be reached by inference. There are two extreme forms of this theory, which I will call respectively the *Purely Deontological* and the *Purely Teleological*. The former holds that there is one, or a set of, self-evident propositions of the form: "Any act having the characteristics C, if done in a situation which has the characteristics T, will be right no matter what its consequences may be." We have merely to analyse the given situation, and satisfy ourselves that it certainly or probably has such characteristics. We can then in every case infer from our axioms that an act of such and such a kind will certainly or probably be right in this situation. Dr. Ewing denies that we either do or could thus determine in every case what is the right act to perform. It is quite certain that *some* weight must be attached to the goodness or badness of the probable consequences of a proposed action in deciding whether it would be right or wrong in a given situation.

The purely teleological theory holds it to be self-evident that the right act in a given situation is that which will produce the best state of affairs on the whole. On this view we must take account of the goodness or badness of the consequences, and we may take account of the goodness or badness of the act itself. (As Dr. Ewing points out, it would be circular for this theory to take account of the goodness of the act so far as this depends on its rightness. But the goodness of an act depends on other factors beside its rightness, and a purely teleological theory may properly take account of them.) On such a view as this, if the right action in a given situation can be discovered by inference at all, the inference must take the following form. We must work out the probable consequences of each possible course of action. We must then analyse each total alternative to discover the good and the bad factors in it. Then we must infer the nett value of each total alternative from the values of the

various factors in it by using some general principle of "summation." And finally we must compare the nett results, and choose that alternative which has the greatest nett value.

Dr. Ewing begins by mentioning and dismissing a number of common objections to this type of theory. As we have seen, it need not deny that acts themselves have some intrinsic value. Nor need it be hedonistic; though, if it is not, it will involve the balancing of different kinds of intrinsic good and evil against each other. It is true that, even if the inferences can be made, the conclusions will never be more than probable owing to the impossibility of getting adequate knowledge of the very remote consequences of our proposed actions. Still, this kind of uncertainty attaches even to astronomical predictions. And the very remote consequences of our actions will depend on so many cause-factors beside the action itself that our responsibility for them will be very much diluted. Lastly, although we certainly do not in fact reach our judgments about the rightness of acts in this way as a rule, it remains possible that we *could have* done so, if we had chosen to, in all cases.

The objection which Dr. Ewing thinks fatal to the theory consists of two closely connected propositions. In the first place, even if the simpler components of valuable wholes have fixed intrinsic values which they carry unchanged from one whole to another, we have no means of discovering these values. Secondly, the Principle of Organic Unities assures us that there is no general rule by which the value of a whole composed of such and such components can be inferred from the values of these components. Algebraical summation is plainly not a safe rule, and no alternative rule has been suggested or seems likely to be discoverable. It seems clear that Dr. Ewing is right on both counts. If, as seems likely, the rightness of an act depends on its fittingness to the initial situation and its subsequent developments, as well as on its intrinsic value and the intrinsic value of its consequences, Dr. Ewing's case is merely strengthened.

We come now to Dr. Ewing's own view. According to him the final judgment as to what action is right is based on a comparison of the nett values of the various total alternatives. And our estimate of the nett value of each total alternative *does* depend on a previous analysis of it into various factors and an estimate of the value of each of these factors. But our estimate of the nett value of the whole is not *inferred*, in accordance with any general principle, from our analysis and estimation of the values of the components. He compares the process to marking examination-papers. It might also be compared to the kind of dependence which subsists between our judgments of distance and sensations of accommodation and convergence in our eyes. It might be called *Mediated Non-inferential Cognition*. He does not deny that in certain favourable cases actual inference by summation may take place; he only denies that anything like this can be used in all cases where we reach a justifiable belief about rightness.

The only serious criticism which I should be inclined to make on this theory is the following. In arguing against the inferential theory Dr. Ewing contended that we do not know the intrinsic values of the components of valuable wholes. If so, how can our final judgments depend, *even in a non-inferential way*, on our knowledge of such values? And how can we *ever* reach our final judgments by inference from such premises even in favourable cases?

Chapters II. to V. inclusive, and Chapter VII., deal with the morality of punishment and reward. Dr. Ewing begins with the Retributive Theory. This theory cannot be proved; but it seems in accordance with the convictions of common-sense, and it has been held in an extreme form by so great a moralist as Kant. On reflexion we can see that retribution is not *sufficient* to justify punishment; but it might be *necessary*. Even this does not commend itself to careful reflexion, and we are left with the tame proposition that the infliction of the appropriate amount of pain on the guilty has *some* intrinsic value, though not a very great one. Dr. Ewing deals with certain arguments which have been brought against even this attenuated form of the Retributory Theory, and concludes that they could be answered. The main objection is that pain and sin are both evils, and that two evils cannot make a good. Three answers can be made to this. (i) It might be that a certain relation between two evils is intrinsically good. (ii) It might be that the act of punishing is intrinsically good, though the pain which is its inevitable consequence is evil. (iii) In accordance with the principle of Organic Unities it is possible that a whole composed of sin and a certain amount of pain might be better than the sin alone or combined with pleasure. (iv) If we do not accept a purely teleological view of rightness it is possible that the act of punishing sin is *right*, even though the consequence of it is not intrinsically good. Still, the Retributive Theory does seem paradoxical and isolated from our other ethical convictions.

Other objections are that the Retributive Theory is inconsistent with the duty of Forgiveness, and that it is a mere modification of the evil passion of revengefulness. To the first of these Dr. Ewing answers that forgiveness would be a duty on the milder form of the Retributory Theory, though not on the extreme form which we have already rejected on other grounds. To the second he replies that "Righteous Indignation" might be a sublimation of Revengefulness and yet be a quite different and morally higher emotion.

But, if the milder form of the Retributory Theory cannot be positively refuted, retribution cannot be used in practice as a principle of punishment by the state. For it is impossible to know the real degree of guilt involved in a criminal act, or the criminal's susceptibility to painful stimuli, or the exact amount of pain which is appropriate to a given degree and kind of guilt. It is plain that the state cannot punish all moral faults, though on the Retributory Theory they all deserve punishment. And the state cannot determine on

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purely retributive principles which faults it shall punish and which it shall ignore. In punishing the guilty the state cannot avoid causing pain to their innocent friends and relations, and thus doing wrong on the retributory principle. And, if it punishes a crime either too much or too little it will be doing wrong on this theory. So, on purely retributory principles, the state would hardly ever be justified in punishing at all, in view of the uncertainty of ever doing right and the certainty of often doing wrong.

At this point it seems to me that Dr. Ewing's arguments become rather wire-drawn. He admits that in many cases we seem to be able to recognise upper and lower limits within which the appropriate amount of pain for a given crime would lie. And surely he has no right to assume that, on the retributory principle, there could not be degrees of appropriateness of punishment to crime. It is fantastic to maintain that, on the retributory principle, it would always be better to leave a crime altogether unpunished than to punish it in the least degree too lightly or too heavily.

In spite of his condemnation of the Retributive Theory, Dr. Ewing concludes that there are important elements of truth in it. It is true that punishment ought to be inflicted only for a past offence; that it ought not to be inflicted by the state except for breaches of laws which were in force at the time when the act was done; that a lighter offence should not be punished more heavily than a greater one; and so on. Each of these goods *may* on occasion have to be sacrificed for greater goods; but the sacrifice is always a serious one. The question is whether any other theory of punishment will account for these ethical facts.

In Chapter III. Dr. Ewing is mainly concerned with the defence of punishment as a means of deterring others beside the criminal from breaking the laws. Such punishment does involve treating the criminal as a means, though it is not inconsistent with also treating him as an end. And if we do not punish criminals we treat the innocent as means to their comfort. But if we tried to defend punishment on *purely* deterrent grounds, we should be involved in extreme ethical paradoxes. We might have to punish more severely where the temptation was greater; to punish impulsive crimes more severely than deliberate ones; to punish mere carelessness more severely than actual crime; and to punish people whom we knew to be innocent provided they were popularly believed to be guilty. On the whole, if the degree of punishment were determined on purely deterrent grounds, punishments would tend to be excessively severe; though there are arguments against extremely severe punishments, even on these grounds. The deterrent effect of a punishment is largely due to its being considered disgraceful, and it does not increase proportionally to increases in its severity. Extreme severity turns popular sympathy in favour of the criminal; tends to brutalise both the general public and the officials who have to inflict it; and tends to make the criminal regard himself as a

martyr. But, even if these considerations would in practice mitigate the paradoxes of a purely deterrent system of punishments, the theoretical inadequacy of the theory would remain. It does treat the criminal and the rest of society merely as animals sensitive to pain, and hardly as rational and moral beings. If only the guilty should be punished, and if the more guilty should never be punished less than the less guilty, this fact would be purely derivative and extrinsic, whilst it seems to common-sense to be intrinsic and ultimate.

Dr. Ewing's own positive theory of the essential nature of punishment is contained in Chapter IV. on "Punishment as Moral Education." Punishment may, of course, "reform" the criminal, in the sense of merely frightening him from committing illegal acts in future. Again, it may, if it involves detention and supervision, enable him to be subjected to other influences which will produce a genuine reform in his moral character. But the important questions which are discussed in this chapter are the following: Does punishment itself have any tendency to reform the criminal's moral character? And does it have any tendency to improve the moral characters of other members of society? If it does, these may be called the *Educative effects of punishment*.

Dr. Ewing has little difficulty in answering the objection that, since dislike of present and fear of future pain are not moral motives, punishment could never produce moral reform. A non-moral cause might produce a moral effect; and, in any case, the non-moral motive is only one cause-factor and not the total cause. Again, punishment might at least diminish the obstacles to future right willing; and a habit of externally right conduct which was formed by non-moral causes might come to be appreciated and continued against future temptations on higher grounds. Lastly, the moral motive may first get a chance to act when a strong counteracting motive has itself been neutralised by the fear of pain. And, in point of fact, punishment not only may but sometimes does produce moral improvement in the criminal. For it may enforce on his attention the fact that his action has been wrong by showing him that it is branded as wrong by society. Nevertheless, there are so many ill-effects which punishment can have, and the chance of producing direct reformation is so slight, that the state cannot make the direct moral reform of the criminal its main object in punishing him. In the education of children, however, the motive of moral education by punishment can be allowed to play a larger part. But, even here, the verbal expression of disapproval without punishment would often, though not always, be better on the whole.

We come now to the educative effects of punishment on society at large. When an act is punished by law the public realises that it is seriously wrong. Men tend to divide acts which they believe to be wrong into "wrong" and "very wrong." When the state punishes an act which is really wrong men tend to put it into the second class. Most men, except under extreme provocation, never

seriously contemplate the possibility of doing acts which the state has branded as crimes; and this enables them to reserve their energies for fighting more subtle temptations. Any unpunished crime tends, by example, to undermine morality; and, although punishment cannot "annul" a past crime, it can do something to annul the present evil effects which a crime produces by its bad example.

We come finally to Dr. Ewing's own view of the essential nature of punishment, and of the truth contained in the Retributory Theory. His doctrine is as follows. The infliction of pain is a *natural sign*, recognised as appropriate by everyone, of *moral disapproval*; and the infliction of greater pain is a natural sign, at any given period in any given society, of greater moral disapproval. Pain, as such, is not appropriate to sin, as the Retributionists think; but the infliction of pain is the appropriate expression of disapproval of sin, and the suffering of pain is the appropriate sign of the defeat and the essential worthlessness of sin.

This theory, Dr. Ewing thinks, accounts for all that is true in the Retributory Theory, whilst it enables us to avoid the paradoxes of that view. Disapproval of evil is an intrinsically good state, and it is an intrinsically good thing that this state should have its appropriate external expression. It is an evil that the innocent should be punished or the wicked rewarded or that trifling sins should be more heavily punished than serious ones, because all such injustices are false and misleading expressions of the moral facts. As such, they are both intrinsically evil and evil in their consequences. The advantage of the theory over the Retributive Theory is this. It can recognise that, as men become more intelligent and more sensitive morally, a lighter punishment may express as great moral disapproval as a heavier one did at an earlier period, and therefore may be equally appropriate to the same crime. And it can look forward to an ideal future situation in which, without sacrifice of justice, all special infliction of pain might be replaced by mere censure and the inner pain which is essentially bound up with the experience of being blamed and knowing that the blame is deserved.

In Chapter V. Dr. Ewing deals with the morality of Reward on the same general principles as he has used in dealing with Punishment. There is no need for me to go into the details of this chapter. Like the rest of the book it is full of excellent ethical and psychological observations.

I have, I hope, said enough to show that Dr. Ewing is to be very heartily congratulated on a work which is both sound and illuminating in theory and of considerable importance in its possible practical applications.

C. D. BROAD.

Ethics. By F. C. SHARP, Professor of Philosophy, University of Wisconsin. New York: The Century Co., 1928. Pp. vi, 566. \$3.50.

WHETHER or not they can feel perfectly happy in his ultimate conclusions, all moralists who have had the good fortune to read Prof. Sharp's *Ethics* will feel under a great obligation to him. And perhaps a large share of the intrinsic worth of the book is due to the manner in which Prof. Sharp has approached his fundamental problems. Most of us acknowledge that any valid ethical theory ought to start, as far as possible without distorting presuppositions, from observation of the actual facts of moral experience. But as a general rule we are apt to confine our observation to a narrow field—our own moral consciousness and the experience of a small number of intimate, and consequently like-minded, associates. We claim, however, to present a theory of the moral consciousness in general; and Prof. Sharp has begun with the conviction that, in order really to justify such a claim, the writer on ethics must widen the field of his preparatory observation. Accordingly, the author has gone to history and anthropology; and, with the co-operation of numerous other persons (including his own colleagues and pupils), he has made careful and systematic 'moral experiments,' in order to elicit *data* for analysis and theoretical reconstruction.

This method is not, indeed, entirely proof against the influence of preconceptions. Prof. Sharp speaks of many of those interrogated as "of course, entirely innocent of any acquaintance with theories of ethics" (p. 33). But, if we do not arbitrarily confine the term ethics to scholastic dissertations and lectures, it is hardly possible to suppose that many persons having entered the years of adolescence are innocent of theories of ethics in some more or less crude form. To make this obvious point may seem unnecessary, but it has to be remembered that the *data* Prof. Sharp aimed at eliciting were "beliefs about, or judgements upon conduct" (see p. 493); and judgement, especially if it be passed upon some situation which has been deliberately presented as a 'problem' for solution and carries no immediate practical consequences for the person judging, is almost bound to be a highly 'theoretical' judgement. Further, the person who sets the problem is not uninfluenced by theory. His 'experiments' are made in order to test some principle, and his questions are put forward, and certain aspects of them emphasised, because of the end he has in view. I may be misunderstanding, but Prof. Sharp sometimes conveys the impression that he thinks the best (or, at least, the safest) subjects for moral experiment are those who are unacquainted with academic ethics. I am suggesting that this is not so, and that ethical theory so permeates all moral judgement that we cannot get away from its influence in even the most rudimentary (or incoherent) moral consciousness.

This caveat, although it may not agree with the letter of Prof. Sharp's contention (and of that I am not quite sure), is certainly in

line with the spirit of his work. It only goes to emphasise the value and necessity of wide and careful observation such as that undertaken by the author. And not only is the material collected by him very skilfully used to further his main purpose, but its inclusion would almost have been justified by its informative and interesting qualities alone.

It is only fair to recognise, however, that very many moralists would sympathise with the author's rather than with the reviewer's sentiments on the question of the possibility of discovering judgments passed by a moral consciousness uncontaminated by ethical theory; and perhaps I may be allowed to detail one of Prof. Sharp's questionnaires, both for its own interest and also because its wider implications may give some genuine support to his conception of a moral consciousness innocent of ethics.

The particular problem¹ investigated by the author which I shall select concerns the influence of 'authority' on the moral judgement. The subjects consisted of 100 students in the first year of the Short Course in Agriculture in the University of Wisconsin; and they were asked to say whether private vengeance is justified under circumstances such as the following:

"The scene is laid in the mountains of eastern Kentucky. A physician, widely beloved and trusted, who had lived his whole life in that region, had been called in to attend some men who had been seriously wounded in a blood feud. Soon after he was met on a lonely road by one of the feudists. The latter informed the physician that, in revenge for the services rendered to his enemies, he was about to kill him. The doctor pleaded for his life, not for his own sake . . . but for his wife and family. In reply the murderer only laughed at him, and, after rendering escape impossible with a single shot, proceeded in leisurely fashion to shoot him to pieces, making the less vital parts of the body his first target in order to lengthen his victim's agony as much as possible, jeering at him the while between the shots. Provided that conviction by a court of law was impossible, would one of his relatives be justified in avenging this death? It being distinctly understood that the return of like for like would do nothing to prevent such deeds from recurring."

When this casuistry question had been put, the students were asked if they believed that the teaching of Christ represented an infallible revelation given to man by God. Fifty-six answered in the affirmative; and on being further asked whether, in view of the prohibitions of Matthew vi., revenge was justifiable, forty of them (some less and some more reluctantly) condemned revenge. But sixteen of the fifty-six stood unmoved champions of retaliation. "It may be alleged," says Prof. Sharp, "that those who refused to recant were persons whose belief in the Bible was purely nominal, . . . but this was demonstrably untrue of at least half this number. . . . One young man I shall never forget. He sat before me with his head partially bowed, beads of perspiration standing on his forehead.

¹ Pp. 208 ff.

In as many ways as my ingenuity could devise, I placed before him the incompatibility of revenge and the Sermon on the Mount. Yes, the Bible is the word of God, and true from cover to cover; 'but this man he had ought to be punished. I ain't got nothing to say against the Bible, but that is the way I feel.' Such was his constantly reiterated reply." "Authority," Prof. Sharp concludes, "is simply one agency among others." Authority may decide where there are no strong convictions; but where a man's ideals are deeply rooted in his character, its claim to be the determinant of moral judgement will often be, perhaps unconsciously, repudiated. How far this investigation also carries implications with respect to the importance of ethical theory in determining the moral judgement, it is not easy to say.

Passing to more general and fundamental topics. If Prof. Sharp is to be placed in any 'school' it is that of Utilitarianism. In his introductory chapter on the "Problems of Ethics," he tells us that the subject-matter of ethics is supplied by two different but intimately related terms, the 'right' and the 'good.' The notion 'right' refers to actions, the notion 'good' having a much wider significance. Accordingly, he divides his work into two Books—Book I. dealing with Right, and Book II. with Good. The fundamental questions raised by judgements of 'right' are: (1) What actions are actually regarded as right? (2) What actions are really right? (3) What do we *mean* by 'right'? Similarly, judgements of 'good' raise the questions (1) What is actually regarded as good? (2) What is really good? (3) What do we *mean* by 'good'?

I. (1) The 'actions actually regarded as right' are of course Prof. Sharp's principal *data* which he has derived from his experiments and questionnaires. (2) The second question Prof. Sharp answers in his formulation of the *standard* of right (or standard of morals). "That action is right which aims to bring into existence the greatest amount of good for all concerned attainable under the conditions" (p. 140). (3) The final formulation of the *meaning* of 'right' is not arrived at until after the 'good' has been discussed, but the provisional definition is as follows: "We might . . . define right conduct as that which is desired when it is looked at from an impersonal point of view" (p. 109). "... When I say 'This conduct is right' [I mean] 'This is the conduct I desire (p. 109) . . . when I have eliminated all the accidental relations of self to the parties concerned; [for it then] represents the way I wish A to treat B, whoever A and B may be (p. 110).'"

II. (1) Again, of course, Prof. Sharp presents the results of his empirical investigations as the answer to the question, 'What is actually regarded as good'? (2) It is rather difficult, however, to say how Prof. Sharp answers the question, 'What is really good'? In the first part of his work he is so clearly at one with the critics of Psychological Hedonism and all forms of Egoism that it comes as a distinct surprise when, at the end of Book I. and

throughout Book II., he avows himself an Ethical Hedonist. The general answer to his second question concerning the good is that *pleasure* is the only ultimately and really good thing; but I am not sure that he holds to this position consistently. One can begin to see, after the first shock of surprise, why he adopts the standpoint of Ethical Hedonism; and one might even go so far as to say that in so doing he is only following out some of the implications of his Utilitarianism. But while to some extent the tyranny of consistency makes him a Hedonist, it does not (so far as I can see) succeed in making him a consistent Hedonist. Pleasure is variously described as the *content* of the good (that which really is good), (p. 520); as the *standard* of the good (see index, under 'Good'); and again as the *harmonising principle* (p. 519) to which reference is made in the immediately following definition of the *meaning* of 'good.' (3) "Good . . . can only mean what is desired when reflection has led me to reduce the original chaos of desired objects to a self-consistent or harmonious system" (p. 410).

Having dealt very fully with 'good' and 'right' in themselves, Prof. Sharp reaches the conclusion that the moral judgement as a whole involves a judgement of right and a judgement of good. "The moral judgement which pronounces a volition right or wrong is really a fusion of two judgements . . . (i) It asserts that a certain 'state of things,' regarded as attainable, is valuable or good. (ii) It locates the good, affirming that it is right—or wrong—that A [or B] should possess it. If a good is within the reach of A, the pursuit of it on his part can be wrong . . . only because it is incompatible with some greater good, whether of A himself, or some other individual . . ." (p. 451).

Hence we get our final formulation of the standard of right. "That act is outwardly right which produces the greatest amount of pleasure attainable under the circumstances under consideration. . . . The aim to produce such a state of things is inwardly right, or right in the proper and moral sense of the term" (p. 452).

Before we discuss these fundamental doctrines, we may advert to an emphatic distinction drawn by Prof. Sharp between the *definition* of right and the *standard* of right (p. 107). The positive value of a philosopher's work so often depends upon his having noted a variety of problems where the generality of thinkers had failed to discriminate, that any distinction emphasised by an accredited thinker is entitled to respectful attention. And yet (although I confess that at first I was inclined to accept it as valid and important) I very much doubt whether the distinction between 'definition' and 'standard' (as drawn by Prof. Sharp at any rate) is really significant. If we *define* 'right conduct' as 'conduct possessing the characteristics A, B and C,' surely the possession of those characteristics is the *standard* by means of which we judge conduct to be right? What professes to be an argument for the necessity of such a distinction, is in reality no argument at all. According to Prof.

Sharp's view (p. 107), if one asserts with Aristotle that those actions are right which represent a mean between two extremes, one has pointed to an alleged standard, but one has not given a definition. But surely a definition (right or wrong) is just what has been given. 'A right action *means* an action which represents a mean between two extremes.' Again, Prof. Sharp tells us (p. 107) that "To say that right *means* 'procuring the greatest happiness for the greatest numbers' is [only] to say 'Those actions procuring the greatest happiness of the greatest numbers procure the greatest happiness of the greatest numbers'." Apparently he thinks this conclusive proof that the above cannot really be a 'definition'. But why not? What is a definition but a precise translation into familiar terms of a term whose significance is not *prima facie* evident? Any definition must be capable of being expressed in the form, "Right (conducive to the greatest happiness of the greatest number)" *means* "Conducive to the greatest happiness of the greatest number (right)."

I should not have so enlarged upon the foregoing topic were it not that (as it seems to me) Prof. Sharp's attempt to distinguish between 'meaning' and 'standard' leads him, in the end, into some confusion in his general account of value or goodness. We have already noticed that Pleasure is spoken of sometimes as the *content* of the good, sometimes as the *harmonising principle* of the good, and sometimes as the *standard* of the good. As Prof. Sharp is quite well aware of the fallacy in Psychological Hedonism, it would be rash to say outright that he himself is guilty of similar confusions in thought; but one is puzzled to know how the following passages are to be interpreted:

"... *Pleasure*¹ constitutes the sole element of *value* [is the sole *good*?] in experience, although it is not the sole object of direct desire" (p. 411).

"*Any* given experience or element of an experience is *good* [possesses *value*?] . . . in so far as its actual or accurately imagined possession either gives me satisfaction or would do so if its possession became an object of thought . . ." (p. 518).

Calling to mind the fact that these statements are made by one who is a Hedonist (who holds that the ultimate *object* or *content* of experience valued is pleasure), we may be pardoned for suggesting that Prof. Sharp has not consistently observed the distinction between the questions (1) What is good-ness—what does 'good' mean? and (2) What (object) *is* good? And if it be the case that these questions are occasionally confused by the author, I should look for the explanation in this, that, drawing an untenable distinction between 'meaning' and 'standard,' Prof. Sharp has somehow given his 'standard' the status of an object valued; and yet, just because his 'meaning' and 'standard' are not really distinguishable, the conception of 'standard' has only served as an unfortunate bridge

¹ All italics mine.

between the notions 'meaning' and 'object,' and has therefore caused him to blur the distinction between them. If this be a fair criticism, it is to the effect that the author's lively sense of the necessity for emphasising real distinctions has, through an unfortunate slip, caused him to obscure a very important one.

These tentative remarks are offered, not so much as definite criticisms, but rather as indicating a certain doubt in the reviewer's mind as to how far Prof. Sharp has really evaded the classical criticisms of Hedonism. Now whether or not Prof. Sharp's emphasis on pleasure as the sole good can be defended apart from the acceptance of Psychological Hedonism, it is perfectly clear that that doctrine, as such, does not commend itself to him. Why, then, does he defend a form of Ethical Hedonism? I think that light can be thrown on this question only when we recognise the nature of the fundamental cleavage between a Utilitarian and a non-Utilitarian theory of political and moral obligation. And perhaps this cleavage can be shown most clearly by comparing the position of Prof. Sharp with that of one who was also (nominally) a Utilitarian and Hedonist—J. S. Mill.

Mill, as I understand the matter, took over two leading ideas from Bentham—the Hedonistic principle and a somewhat legalistic theory of Justice. According to Mill, justice arises where different individuals are living together in a society, each 'pursuing his own happiness'. Justice consists in each person's pursuing his happiness in a manner such as to leave equality of opportunity for all others to pursue their own happiness also. Thus, although he speaks of justice as 'pursuit of the general happiness,' it is clear from his argument that he thought of justice in a much more negative way than 'pursuit of the general happiness' implies. Justice is not the positive *promotion* of the general good, but rather the leaving scope for others to pursue what they themselves consider to be their good.

Thus, it is obvious that if 'Utilitarianism' is the theory that "morality is essentially the positive promotion of good" (the good of self and others), Mill was not strictly a Utilitarian.¹ And it is because he was not a Utilitarian in the strict sense of the word, and because he implies the existence of a disinterested respect for other persons, that Hedonism was so absolutely at variance with his central ethical position.

Now the main difference between Mill and Prof. Sharp is, I think, that the latter is much more faithful to the principle of Utilitarianism, as defined in the last paragraph. On Prof. Sharp's view, we may perfectly well adopt a policy of 'non-interference,' but we shall do so only because we see that to be the best way of promoting the good of others in certain circumstances. On the other view, we may perfectly well 'promote' in a positive manner the happiness of

¹ It may reasonably be argued, on historical grounds, that the special sense in which I understand the term 'Utilitarianism' is inappropriate; but I trust that the meaning I give to the word is, at all events, clear.

others, but that is because we see that under certain circumstances such positive 'promotion' is really implied in the 'leaving scope for others to pursue what they conceive to be their good'. On Prof. Sharp's view, the 'positive promotion' is the essential thing, and others have rights against us because it is our duty to pursue the good. On the other view, the 'non-interference' is the essential thing, and we owe duties because certain others hold rights against us.

The 'non-interference' theory can give some practically significant meaning to the conception of 'right conduct,' and yet rest perfectly content with the view that 'goods' are many and different because men's needs and natures are different. But to give the same degree of practical significance to the notion of 'right conduct,' the 'positive promotion' theory is obliged to point out certain definite things or 'states of things' which *are* good; and, in order to provide for cases in which two goods conflict, this latter theory is almost compelled to "reduce the chaos of desires to an harmonious system" and to embrace some such conclusion as, "the content of the good must be declared to be pleasure" (p. 403); this latter theory, in other words, is almost compelled to discover some object which is absolutely and always good.

I find Prof. Sharp's Hedonism, then, to have an important place in his system. For Mill, Hedonism was a relic, a presupposition with which he started but with which his developed doctrine was quite inconsistent. For Prof. Sharp, Hedonism is a doctrine to which he finds himself driven in the attempt to work out his Utilitarian theory of morals.

Perhaps this review of Prof. Sharp's work is misleading in the sense that it may suggest the existence of more difficulties and possible errors than merits, in the book. But if that be the impression conveyed by this notice, the fault should be charged to the reviewer's defective sense of proportion, or (perhaps more charitably) to his having temporarily adopted the *persona* of Devil's Advocate as against the claims of a strong candidate for canonisation.

It only remains to be noted that, typical of the completeness so characteristic of the work in general, Prof. Sharp has supplied a full index, and has appended a carefully compiled bibliography.

W. D. LAMONT.

Gestalt Psychology. By WOLFGANG KÖHLER. New York: Horace Liveright, 1929. Pp. 403. Price \$4. London: G. Bell & Sons, 1930. Pp. xi + 312. 15s.

THIS is a book of great interest and importance, for it concisely and lucidly sets forth some of the main conclusions of *Gestalttheorie*, together with the arguments and evidence on which they rest. It makes valuable contributions to the store of psychological facts,

and even more valuable contributions to psychological theory. We have long known that Köhler is an enterprising and able experimenter; we now know that he is also an excellent reasoner.

Consider the fifth and sixth chapters which deal with "Sensory Organisation" and "The Properties of Organised Wholes." Köhler here seeks to establish the view that in every sensory field there are parts which 'belong together,' and that the segregated wholes, or *Gestalten*, which are formed in this way, are immediately given. He thus sets out to destroy a much-worshipped idol. For many people hold that there are no segregated subordinate wholes within any sensory field except those which emerge under the pragmatic influence of past experience—or, in other words, that acquired meanings are entirely responsible for all sensory organisation. This theory is indeed often held both by those who believe that every sensory field, as it is immediately given, consists merely of a chaotic mass of atomic sensa, and by those who agree with James that throughout every sensory field, as it is immediately given, there is uniform continuity.

Köhler begins by looking at his desk and finding "quite a number of circumscribed units which appear detached and segregated in the field: a piece of paper as against the surface of the desk, a pencil, an eraser, a cigarette, and so forth". And he is quite ready to admit that all these objects are familiar to him; that they are full of 'meaning,' and that he has had more than ample opportunity of learning their uses and names. "But from these facts," he says, "there is a large step to the statement that neither the paper nor the pencil would exist *as segregated units* in my visual field without that previous knowledge about their practical behaviour and use. It may be that before I had that knowledge the same things occurred in the sensory field as units, unknown and unnamed, but still as segregated wholes. When I see a *green* object, I can tell the name of the colour immediately; and I know that green is used as a signal on railroad tracks and also as a symbol of hope. But I do not believe that, therefore, the colour green *as such* must be explained by meaning. Existing independently it has acquired several secondary properties in my lifetime and I agree with the reader in praising all the advantages which this kind of learning holds for all of us. In exactly the same manner [other] sensory units may have acquired names and may have become richly symbolic in the context of our knowledge, while existing, nevertheless, as segregated units in the sensory field prior to such accretions. Such is the conception which *gestalt* psychology offers to defend" (p. 151).¹

Köhler's own defence consists in reviewing a formidable number of actual cases of sensory organisation which the 'past-experience' or 'meaning' hypothesis seems unable to explain. Three of these cases may be briefly described: (i) Let a person of normal sight and a colour-blind person look at one of those cards which are used in

¹ The pages are throughout those of the American edition.

discovering colour-blindness and are covered with differently-coloured dots at equal distances from each other. The former will observe, as a segregated whole within the field, some particular letter, but the latter will not. The dots which form the letter stand out as a separate group for the person of normal sight because in chroma they all resemble each other and differ from the rest. Now, both persons are equally familiar with letters in general and with the particular letter which the given card bears; their relevant past experience is the same. "Therefore," Köhler concludes, "the striking difference as to grouping depends directly upon sensory conditions" (p. 156)¹. (ii) As soon as a congenitally blind person has been cured of his blindness he knows to what we are referring when we ask him about some object presented to his sense of sight alone. He may fail to recognise what he sees as something which he knows quite well by touch. "Still there is something very positive in his reactions: when asked about 'that something' which he has before him, *he understands the question*. Obviously he has before him something as a segregated unit, to which he refers the question and which he tries to name." From the very beginning the patient has before him an organised sensory field. He does not have to wait upon experience; "he does not have to *learn* what 'aggregate of sensations' he shall 'treat as one thing'" (pp. 162-163). (iii) A familiar figure, rich in 'meaning,' like the number 4 or the letter K, may be so included in an unfamiliar 'meaningless' pattern that we see the pattern but fail to see the figure. Yet, if it were past experience and acquired meaning which controlled the organisation of every sensory field, then in this case the familiar figure should leap to the eye. Köhler remarks that these facts, so often by implication denied, are revealed in all observations of puzzle-pictures and camouflaged objects; and they are clearly established by the extraordinarily neat examples which Köhler himself provides. But it is perhaps just worth while to record some confirmatory evidence which my own laboratory has provided. Forty undergraduates were each given ten cards. On each card one of the numbers between 1 and 10 had been clearly drawn and then deliberately hidden in some unfamiliar and nameless pattern. The subjects were instructed to write down what they saw when they looked at the cards. So far as the difficulty of describing the nameless permitted, the forty verdicts agreed with each other. But in

¹ Strictly, Köhler's 'therefore' is unjustified. From the fact that *AB* causes *C*, while *AD* does not, we cannot infer that *A* plays no part in causing *C*. From the fact that past experience of letters together with normal sight causes the letter to be seen, while the same past experience together with colour-blindness does not, we cannot infer that past experience of letters plays no part in causing the letter to be seen. To complete his proof, Köhler needs to compare two people, each with normal sight, but one familiar with letters and the other not. But there is little doubt that such supplementary evidence *would* complete the proof.

no case did anyone see the familiar number. Indeed, only a few could find it even when they were told what it was.

The essential point of these two chapters may therefore be stated as follows.—We see and distinguish different patterns or forms as immediately as we see and distinguish different colours. It is not necessary to know that green is used as a signal on railways before we can become aware of the presence of green in the sensory field. No more is it necessary to know that a triangle is used as a trade-mark by a certain firm of brewers before we can see triangles and distinguish them from polygons. All sensory material reveals properties of organisation before it begins to acquire meaning. Within every sensory field there are segregated subordinate wholes that are immediately given. Now, these are propositions which are new as well as important. They are not new in the sense that they are not to be found in the earlier writings of Köhler and his colleagues. But they are certainly new in the sense that they have not been clearly expressed elsewhere and have indeed been implicitly denied by those English psychologists who are sometimes falsely supposed to have anticipated whatever is valuable in *Gestalttheorie*. Köhler, like Koffka and Wertheimer, here throws a flood of light on a region of psychology which has long lain in indecent obscurity. And the present volume, like the excellent second edition of Koffka's *Growth of the Mind*, enables English readers to enjoy the effects of this beneficent illumination.

In these chapters Köhler also briefly describes the general principles of spontaneous grouping in sensory fields. And he deals with the properties of organised wholes which he calls *Gestaltqualitäten*, keeping the word *Gestalten* for the organised wholes themselves. In this connexion he remarks that the *Gestaltproblem* in modern psychology began, not with the discovery that segregated sensory wholes are highly important entities, but with the observation that sensory fields are replete with properties—like regularity, harmony, symmetry, slenderness, roundness, simplicity, complexity, etc.—which, pertaining to extensive areas as such, cannot be produced by single local stimuli, and therefore do not fit into the atomistic theory of sensation. It is also worthy of notice that Köhler does not avoid an interesting question which the *Gestalt* view of sensory organisation inevitably raises. "Why," he asks at page 170, "should wholes, detached by the operations of sensory dynamics, correspond so generally to objects, or things, in the practical meaning of the words? Do we have to assume that a surprising harmony is established between the laws of sensory dynamics and the area or the limits of physical things around us?" He answers that in the first place there are countless segregated sensory wholes which correspond to no segregated physical object or thing "in the practical meaning of the words". Plenty of examples may be given of intricate sensory organisation which is immediately experienced in its own right and does not correspond to the contours of what we

in practice regard as separate physical things. Witness the effects of camouflage during the war. Secondly, there are very good reasons why separate physical objects, whether produced by man or by nature, should be accompanied by separate sensory wholes. Objects made by man are prepared for our purposes. "Therefore we give them a form and surface, etc., so that they are likely to be seen without difficulty. Without knowing the principles of sensory organisation in an abstract form, man works in conformity with them, and so the physical units which are the products of his art will appear as visual units" (p. 172)¹. Objects produced by nature also tend to have distinctive "surface properties" which produce distinct sensory wholes. "Even if a stone lies half-embedded in the sand, which is nothing but tiny fractions of the same kind of stone, the difference of coherence, and therefore of 'inner detail,' between the surface elements of the stone and those of the sand will be sufficient to make the stone optically one thing. At least, at the boundary between a natural object and its surroundings some discontinuity of properties almost universally prevails. . . . If there are no such differences and no discontinuity whatever between the objects and its surroundings, no visual object will exist" (p. 173).

We are thus bound to be favourably impressed by this central part of the book. But there are other chapters about which we would ask for what the lawyers call "further and better particulars". This comment does not apply to the earlier chapters, which present an excellent account of the main differences between behaviourism and introspectionism and an able defence of the view that psychology should include the data and methods of both these schools as well as much which both have neglected. It applies to the later and more speculative chapters, where we are introduced to *Gestalten* in which the self and its states occur as functional members. Here, as elsewhere, there is much that is both clear and valuable; but, though I may be wrong, I cannot help thinking that the full significance of these chapters will be grasped only by those who have independent means of knowing all that lies behind them.

The defect of the chapter on "Association" is, however, of a more positive kind. Here Köhler at least lays himself open to misrepresentation. It is true that when he says, at page 293, that "association is given up" by the *Gestalt* psychologists, he is careful to add that it is given up only in the sense that it is not regarded as "a special and independent theoretical concept"; and he elsewhere explicitly says that association is one of the principles on which organisation, or the development of *Gestalten*, depends. But

¹ It is probable, however, that the same considerations apply to artificial objects as apply to natural objects, and that Köhler's special treatment of artificial objects applies only to some of them. A bugle, a bottle and a bicycle are perceived as distinct units, not because their makers were influenced by a desire to secure this result, but for the same reasons that a stone, a sturgeon and a sunflower are perceived as distinct units.

he so often speaks of the "striking differences" between associationism and *Gestalttheorie*, and he so often pours scorn on what he calls "the old rule about association," that many will falsely suppose that he wishes to banish the venerable laws of association from psychological theory. Köhler does indeed bring forward a crushing indictment of that form of associationism for which contiguity, frequently repeated, is the decisive factor. He shows, as others have shown, that the idea of *A* can become associated with the idea of *B* only when *A* and *B* have been perceived as constituents in some larger whole. He points out that what is important is the organisation of experienced objects, not the spatial or temporal juxtaposition of stimuli. And he records many new experiments which show that mere repetition of contiguous items which are not perceived as related is of little avail in establishing associative bonds. But he should not suggest that this amendment of association by contiguity has not been made before; he should be content with the fact that, so amended, association by contiguity forms part of his own wider doctrine of organisation. Nor should he ignore association by similarity. For this type of association, as it was understood by Aristotle and developed by the English empiricists, is also in perfect harmony with *Gestalttheorie*. It certainly does not conform to what Köhler believes to have been "the old rule"—namely, that association is "an indifferent bond between processes indifferent to each other and to the bond" (p. 294). Its very essence is that indifferent items are *not* associated.

In the chapter on "Insight," which is the last of the ten, Köhler, in explicit opposition to Hume, defends the view that we often directly observe the causal relation between two events—or, in other words, that wholes in which the constituents are bound together by causation are as immediately given as other wholes. He brings forward many examples of situations in which, he says, some state of mind is directly ascribed to some event in the field of experience. "When on a hot day I enjoy a cool drink, my enjoyment is felt to refer to, or to be based upon, the properties of the drink and my thirst"; it is not referred to "the spider on the wall, nor to the size of a chair, nor to thousands of other things" (p. 375). Again, "one beautiful night at Tenerife, when I was working calmly at my desk, I was suddenly frightened as I had never before been frightened. The house was rattling and shaking violently—my first experience of an earthquake! There was no doubt whatever about my being frightened *by* that sudden rattling and shaking" (p. 355). In all such cases, according to Köhler, there is insight. The mental attitude is experienced as "*depending directly upon something definite*" (p. 353). We are immediately aware of *ein sachlicher Zusammenhang* between the attitude and its sensory base. We do not have to learn gradually that the one thing is caused by the other. Now, all this is certainly important, and may very well be true. But Köhler sometimes seems also to hold the false view

that the character of an attitude caused in this way is determined solely by the character of its sensory base. There is a hint of this in his example of the concert where he experienced an attitude of admiration directed towards a calm and confident *alto* voice. For he suggests that the character of his attitude was wholly determined by the character of the voice. Similarly, in his first version of the example of the glass of fresh beer after a long summer walk, he ascribes his enjoyment solely to the cool touch and characteristic taste of the beer. But it may be that Köhler does recognise that the character of our attitudes depends upon our own nature as well as upon the nature of that which provokes them. It must be said in his favour that when he returns to the 'drink-and-enjoyment' example in the words previously quoted, he explicitly says that his enjoyment was determined by his thirst as well as by the sensory properties of the drink.

It is to be regretted that Köhler has not clearly related his present treatment of insight to that which occurs in *The Mentality of Apes*. In the earlier book he urged that the solution of problems by insight is quite incompatible with their solution by trial-and-error. But in the sense in which insight is now to be understood no such incompatibility exists. Suppose that *A* is some particular end we desire to attain and that *B* is the means to this desired end. Köhler holds that when we perceive the causal relation between *B* and *A* we do so immediately. The defender of trial-and-error says that it is only by trial-and-error that we come to contemplate *B* at all. And these two beliefs are quite consistent with each other. A concrete example may make the point clearer. Suppose we are attempting to collect precisely seven pints of water and have only a three-pint and a five-pint measure. Now, says Köhler, when we contemplate that use of the measures which solves the problem we are, by insight, immediately aware that it affords the solution. Yes, adds the other, but it is only by trial-and-error that we come to contemplate that particular use of the measures. And however loudly the two parties may speak, there is no inconsistency between their statements. In this connexion it is also noteworthy that among those who believe that all learning is by trial-and-error nobody denies that once the successful variant has been tried it tends to be repeated. Everyone agrees, in short, that when a person has once grasped the solution of a problem no further trial-and-error is needed. And insight, in its present sense, demands no more than this. Nevertheless, I suspect that Köhler still believes that a problem cannot be solved both by insight and by trial-and-error. I also suspect that in support of this belief he would repeat his assertion that when he set his chimpanzees a practical problem they always at first made haphazard movements and then suddenly solved the problem. But this assertion, even if it were true, would not prove that insight and trial-and-error are incompatible. And the assertion is in fact highly doubtful. The suddenness with which the chimpanzees seemed to

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solve their problems may have been merely apparent. The successful action may in each case have been the climax of an unobserved course of improvement in physiological co-ordination. Consider a man who is learning to juggle. If we construct a practice-curve in which his improvement is measured by the number of balls successfully juggled, then, to be sure, before the man reaches the point at which he can handle six balls there will be long periods when the curve will not record any improvement. But this will merely be the result of our having attempted to measure the juggler's improvement by too crude a scale. It will be the fault of the curve. And the suddenness with which Köhler's chimpanzees are alleged to have reached their solutions may be due to a similar cause. For it is to be observed that these creatures did not quickly reach their so-called sudden solutions. There was an interval, often considerable, before they did the trick. If they had solved their problems as soon as the problems were set, then they would certainly have damaged the theory that all learning is by trial-and-error. But in the existing circumstances that theory is still able to hold up its head.

But it would be unfitting to end on a note of criticism. I therefore conclude by adding that the form of this book is no less admirable than its matter. Köhler is to be congratulated on his mastery of a foreign language. His English exhibits an elegant grace which some of our native psychological writings unhappily lack.

REX KNIGHT.

Plato and his Contemporaries. By G. C. FIELD. London: Methuen & Co., 1930. Pp. xi, 242. 12s. 6d.

It is a great pleasure to recommend this instructive and attractive book to all serious students of Plato. The information Prof. Field gives in its three parts about Plato's life, its social and political background, and the Socratic literature generally, is just what any teacher would wish good Honours students to possess; it is furnished by a scholar well-read in the subject; it is amazingly well expressed. The book is a real addition to an English-speaking Platonist's library. For my own part, I own I like the first division of the book—the five chapters which deal with Plato's biography, the Academy, the dialogues—rather better than I do either of the others. (Mr. Field may suspect that this is because some of my own errors are, very politely, exposed and corrected in the later divisions, but I do not think that is the true explanation. I think the fact is that, from the nature of the case, the evidence available for the conclusions reached in this section of the book is more "objective" than that on which the results of the second and third parts has to

depend.) I am glad to see that Mr. Field recognises and fully utilises the *Epistles* for his account of Plato's life—one of the best I have read. (It appears from *Appendix I.* that he suspects Dionysius II. of having foisted in a long passage in the text of *Ep. II.* Candidly, I do not find this suggestion attractive. I do not see why Plato should not have constructed the famous "enigma" of the letter himself, not, of course, as is playfully pretended, to baffle a third party into whose hands the document might come, but as a test of his correspondent's ability to follow up a "hint," a *σμικρὰ ἔνδειξις*, to use the language of *Ep. VII.* And though Plato might conceivably have written of his dialogues as the utterances of *Σωκράτης νέος καὶ καλὸς γεγονώς*, I can hardly conceive that any "forger" would have made him speak so.) The account of Plato's intervention at Syracuse is exceptionally good, and does more justice to the much maligned Dionysius than almost any I have seen before (except that of Burnet in his *Platonism*). Yet I believe even Mr. Field does not say all that can fairly be said for the young man. He forgets to mention the intercepted communication with Carthage which led to the first break between Dionysius and Dion—an example of Dion's extraordinary tactlessness. And I think even the strong proceedings taken at a later stage about Dion's property had an excuse which perhaps imposed on Dionysius himself. No doubt he professed to be acting, as in duty bound, in the interests of Dion's young son, of whose rights he would be the natural protector. I note, by the way, that no use seems to have been made of Eggermann's valuable dissertation on *Eps. VII* and *VIII*, which seems to me to have cleared up the relation between the two letters remarkably.

The account of the Academy is clear and good and makes the right points. But I think it could be made even better in a subsequent edition by adding a few lines explaining the precise contributions of Theaetetus and Eudoxus, and perhaps one or two others, to mathematics. (Cf. the admirable recent pamphlet of H. Hasse and H. Schulz, *Die Grundlagenkrise der griechischen Mathematik*.) I doubt whether there is any evidence in support of Mr. Field's suggestion that Plato, contrary to Burnet's opinion, did nothing to encourage Aristotle's bent for biology. We really know nothing on the point either way, but it is not conclusive to rely on the fact that Aristotle's marine biology is good for the Troas and Lesbos, and apparently draws little from Attica. After all, Speusippus wrote much on biology too, and the remarks of the *Critias* about "denudation" suggest that observational science had its interest for Plato himself. It is part of a polemic against Burnet that Mr. Field expresses a good deal of doubt about the value of any library the Academy may have possessed. I think he forgets that Simplicius seems to have found there the poem of Parmenides, already a very rare work in his day; I do not feel satisfied that his preference for taking the statements of Eudemus about the astronomy of

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Eudoxus at second-hand from an Alexandrian compiler, *proves* that the work of Eudemus was inaccessible to him.

Throughout the second and third sections of the book Prof. Field is frequently recurring to one controverted point to which he gives, as it seems to me, rather more prominence than is strictly relevant to his theme, the question of the historical truth of Plato's account of Socrates. The polemic, perfectly friendly and courteous, though tending, I think, at times to confuse the asking of a rhetorical question with refutation, becomes specially prominent in the third part, and accounts for the addition as *appendices* of two articles reprinted from the *Classical Quarterly*, the first on Aristotle's account of the origin of the so-called "theory of ideas," the other on post-Aristotelian accounts of Socrates and his relation to Plato. Of course, Mr. Field will not expect me to turn this notice into a reply to his criticisms of Prof. Burnet and myself. But I will just say, while thanking Mr. Field for the courtesy of his criticism, that I am a little surprised, not indeed at his dissenting from Burnet and myself, but at his apparent inability to see what Burnet and myself feel as the enormous difficulty of our opponents' position. There are three courses open to our choice in the matter. We may hold that Plato gives a *substantially* faithful account of both the life and the thought of Socrates; that he gives a faithful account of the life, but that the thought is his own; that life and thought alike are a fiction. The third view, that of some recent Germans, Mr. Field finds as incredible as I do; his choice, like that of a good many British students, is for the second. In the main we must believe what Plato tells us about the history of Socrates, but when we come to the "theory of ideas," we are to understand that Plato invented this for himself and that Socrates had neither art nor part in it. But then, I ask, what becomes of the *Phaedo* and *Parmenides*? Both these dialogues not only represent Socrates as expounding the theory of "ideas," but mix it up inextricably with his biography. Both insist that he devised it *for himself* (αὐτός as the *Parmenides* explicitly says) in early life, and the *Phaedo* professes to explain precisely how he was led to think of it; and this piece of professed autobiography is put into the mouth of Plato's hero immediately before he drinks the hemlock. I confess I do not understand how any one who respects the character of Plato can contrive to credit him with so wicked a mystification, or how any one who respects his artistry can believe, except on the very strongest of evidence, that a great artist would encumber himself with such a device. I think Mr. Field somehow contrives to shut his eyes to a good deal of evidence which ought to make him uneasy about his theory. He says—after doing an excellent piece of work in presenting the English reader with a version of the extant remains of the *Alcibiades* of Aeschines—that Aeschines represents Socrates much as Xenophon does, and that he cannot find anything much to treat as evidence in Aristophanes. Now I would ask Mr. Field to consider: (1) that,

both in the *Clouds* and in the *Birds*, Aristophanes represents Socrates as a person with new and mysterious views about the $\psi\upsilon\chi\eta$; (2) that in the *Clouds* he is also represented as the head of a regular scientific school, and that something of the same kind is unmistakably implied by the curious story told by Xenophon—of course it must be something he had obtained from an older source—about Socrates and the *συννομοτῆται* of whom Antiphon tried to rob him; (3) that Aeschines represents Socrates as connected with Alcibiades by a relation which Socrates is made to call $\epsilon\pi\omega\varsigma$ and to compare with the Bacchic "possession," as arguing the thesis (which reappears in *Rep. V.*) of the fitness of women for war and politics, as associating with Pythagorists of the stamp of Telauges. All this is evidence, independent of either Plato or Xenophon, for a Socrates of a very different type from the *brave bourgeois* of Xenophon's recollections, and it is not fair simply to keep silence about such evidence. I have no doubt I have made incidental blunders enough myself; Burnet, a much finer scholar, sometimes made them: but the head and front of our offending is really that we have let the "cat out of the bag" by breaking a conspiracy of silence. I believe if Mr. Field will seriously weigh the evidence collected in Burnet's article *Socrates* in *E.R.E.*, we may yet find him on our side on the main issue.

I have taken this topic rather out of its order, because, as it seems to me, the author allows it to influence his mind in advance in his discussion of the political and social background of the dialogues. His account of the historical conditions during Plato's literary life-time is in general admirable, and ought to be read by every Honours student who is reading Plato. And yet, there is an undertone of polemic running through the whole discussion. The *arrière-pensée* is to demolish part of the case for holding that Plato seriously aimed at preserving a true picture of a wonderful age of which, but for him, we should know nothing, by urging that the moral and political issues discussed by Socrates in the dialogues are really those specially relevant not so much to his own age as to that of Plato the writer. Now, partly, this contention is true but not true in the way necessary for Mr. Field's purpose. In a work like the *Gorgias*, or the *Republic*, the main moral interest is to defend moral principles which are *aeternae veritates*, as vital in any one age as in any other. That it is "better to suffer wrong than to inflict it" is a truth relevant to the fourth century B.C., the fifth, the twentieth century A.D. alike. In another sense, the thesis seems to me to be relevant to Mr. Field's contention about Plato and Socrates, but not true. *E.g.*, there is plenty of evidence—Euripides alone would prove the point—that the doctrine of the *Wille zur Macht* was really fashionable early in the Archidamian War, the time when we are apparently supposed to think of Callicles as defending it in the *Gorgias*. But the *Gorgias* was certainly written between 399 and 390 when Athens was helplessly at the mercy of Sparta;

is it likely that theories like that of Callicles were the popular thing with the *intellectuals* just then? Mr. Field tries to show that there were those who thought thus "in their hearts" then also. But that is not really the point. I would urge every one to read all Mr. Field has to say most carefully, but then to ask himself whether, even if we grant it all, he has said anything whatever that shows that the theories canvassed in the dialogues are not strictly relevant to their assumed dramatic dates. It is true, for example, that the value of the professional soldier was a real topical issue when Plato was writing the *Republic*. But if Mr. Field and myself are right about the dramatic date of the *Republic* something had happened shortly before to make the same topic a "live" one when Socrates and Thrasymachus met. Socrates is drawing the right lesson from the success of Demosthenes and Cleon at Sphacteria.

On the problem of actual definite "anachronisms" in the dialogues Mr. Field is very rational. I am not absolutely convinced that the use of the verb *διοικίζειν* in the well-known passage of the *Symposium* proves that the words were written after the *διοικισμός* of Mantinea. If Plato is alluding to the events of 418, he was a boy of not more than ten when they took place, and his recollection of what they were may not be perfectly accurate. Or again, the Spartan treatment of the Arcadians in that year may have included some harsh measure of which we have no record. I am glad that Mr. Field challenges the presence of allusions to Ismenias and the "Persian gold" in the *Meno* and *Republic*. Ismenias was naturally a common name at Thebes, and it seems to me, as it did to Burnet, improbable that the Ismenias of the *Republic* belongs to the fourth century at all. The reference in the *Meno* is clearly to a contemporary of the conversation, and thus may well be to the Ismenias ultimately murdered by the Spartans; but it seems to me impossible to bring "Persian gold" into it. The meaning clearly is that Ismenias, whoever he was, had recently suddenly come into a fortune. This will not fit the known facts about the "gold of Persia".

I should wish, however, that Mr. Field would reconsider the main question whether he has not, for a controversial purpose, unduly underrated the very real differences in the economic and political situations before 404 and after, and I should be willing that the issue should be decided, *e.g.*, by the picture given in E. Meyer's *Geschichte des Altertums*. I own that Burnet occasionally exaggerated on the other side. Once more let me recommend all lovers of Plato to give themselves the enjoyment of reading Mr. Field's book.

A. E. TAYLOR.

The Quest for Certainty: A Study of the Relation of Knowledge and Action. By JOHN DEWEY. Gifford Lectures, 1929 (at Edinburgh University). London, George Allen & Unwin, Ltd. Pp. 302. 10s. 6d.

PROF. DEWEY is not content merely to marvel at philosophies as miraculous efflorescences of human ingenuity; he insists on knowing them by their fruits, whether they be of the God of Truth or of the Father of Lies. And, what is even more disconcerting, he is also a *radical*, who loves to dig down to their roots in the social conditions out of which they grew, and delights to drag forth into the light of day the hidden motives which, often unconsciously, determined their structure. This procedure, and the revelations it entails, is naturally resented by the philosophers; but it is very effective in what is (in American) called 'debunking' the tradition, and in rationalising and rendering intelligible the history of philosophy.

His present volume also is full of fine spade-work of this sort. Carrying on a valuable line of thought begun in *Experience and Nature*, where he had shown up the connexion between the Greek esteem for 'contemplation' and the social institution of slavery, he sets himself to trace the nexus between the quest for 'certainty' and the distressing insecurity of primitive existence. Inasmuch as human control of the conditions of life was so imperfect and human power to predict the course of events so limited, it was imperative to create some ideal refuge from the actual in a metaphysical realm, in which pure thought could grasp ultimate reality with absolute assurance. Its contemplation could then afford what was really a religious satisfaction, and by comparison with it all the activities of mere mundane making and doing paled into insignificance.

So there arose a *double* world (held together by a metaphor or a myth), one mutable and unstable, engaged in Becoming, and the concern of the senses, the other one of 'eternal' changeless Being, apprehended by reason. The one was the object of practice and opinion, the other that of theory and true knowledge. The value of the latter, of course, stood much higher, and was expressed in respect for 'philosophy' and useless knowledge, and contempt for mere practice. This valuation passed from Greek philosophy to the Christian Church when the latter identified the 'contemplative' with the monastic life, and has lasted down into our time.

But Prof. Dewey contends that ever since the rise of the experimental method it has really been antiquated. For modern science has shown that the way to *know* scientific objects is not to 'contemplate' them but to *operate* with and on them; moreover, rightly understood, scientific conceptions are all *instrumental* and *tools* in re-fashioning the real world of immediate experience. This revolution has proceeded gradually, and was completed only in our day by the dethronement of the Newtonian physics which, by smuggling in "intrinsically unchangeable" atomic substances, had preserved an essential dogma of the old metaphysic; thus

had "Newton foisted a fundamental 'rationalism' upon the scientific world all the more effectually because he did it in the name of empirical observation" (p. 200). But now the one premiss which has been common to science and the old philosophy is abrogated, and with it the whole antithesis of theory and practice is overthrown. Both the sensationalist and the rationalist theory of knowledge had assumed that "the true and valid object of knowledge is that which has being prior to and independent of the operations of knowing" (p. 188). Thus the end of knowledge was fixed before any knowing began, and knowing was merely acceptance of an already extant reality. Neither of them realised that experiment *alters* existences and manipulates reality. "Experimental knowledge is a mode of doing" (p. 100), and "what is known is seen to be a product in which the act of observation plays a necessary rôle. Knowing is seen to be a participant in what is finally known" (p. 195).

Prof. Dewey very prettily shows that this is the philosophic implication of Heisenberg's 'principle of indeterminacy' (p. 192 f.). It finally disposes of the prejudice that knowing may not alter its object, relieves the Newtonian scheme of the mutually contradictory postulates that the position and velocity of a particle can be determined in isolation while nevertheless all particles are in continuous interaction (p. 193), and so dislodges "the old spectator theory of knowledge" (p. 196). For we need no longer hold that "the act of observation, necessary in existential knowing, is proof that the act of knowing gets in its own way, frustrating its own intent" (*ibid.*). The conception of natural law is changed, and "the individually observed case becomes the measure of knowledge" (*ibid.*). Laws are "intellectual instrumentalities" and "*formulae for the prediction of the probability of an observable occurrence*" (p. 197). A 'block' universe, which "has no place for unique and individual existences, no place for novelty and genuine change and growth" (p. 200), is no longer thinkable; "the sacrifice of the individual to the general" is no longer called for, and the qualities and values of human experience as we live it are reinstated as of right.

After that one would fain believe that we had really heard the last of the old philosophic bag of verbal conjuring-tricks. We ought, at any rate, to hear no more gush about the inherent sanctity of 'theory' and its undying antagonism and noble indifference to 'practice.' But long experience of the psychology of philosophers renders me less sanguine. Intellectualism has no doubt met its Waterloo. *La vieille garde meurt*—but with *le mot de Cambronne* on its lips. And so long as the record stands and the attempt persists to teach philosophy by way of its history, all the failures of the past will continue to be rehearsed in the name of 'culture,' and conscientious efforts to *understand* a Plato, a Spinoza, a Kant, a Hegel, will always entail re-immersion in the confusions in which they were steeped. Moreover, Prof. Dewey's charges, though sound, are made in very general terms. They stand in need of copious illustration

by detailed studies all through the history of philosophy. I am filled with awe and enthusiasm at the thought of the multitude of Ph.D. theses in philosophy by which they might be elaborated and substantiated and proved up to the hilt against every notable philosopher. There is probably a hundred years of profitable employment for academic industry in Prof. Dewey's book.

It remains to note the very interesting application of his principles which Prof. Dewey makes to the problem of values in his antepenultimate chapter and which forms their most novel development. He points out that one consequence of taking the methodological abstraction from values in science, not instrumentally, but as a denial that ultimate reality had any relation to human values, was that philosophy was forced to a hard choice between recognising, uncritically, all the values any one might claim to enjoy and skying 'eternal' values in an imaginary realm beyond existence, and making their connexion with human values unintelligible—with a resulting paralysis of ethics. But why, he asks, should not the experimental method be applied also to ethics, politics and economics? We have already a good deal more knowledge about these matters than we try to use, and by experimenting could find out more.

Viewing Prof. Dewey's work as a whole I have nothing but admiration for the sustained and unrelenting vigour with which its argument is driven home, and am at a loss to find anything to disagree with. I was a little surprised not to find the word 'fiction' applied anywhere to the conceptions with which the natural sciences operate. For the importance of fictions in scientific procedure has been exhaustively exhibited in what is the most radical, or perhaps the most extreme, form of the pragmatic movement, that represented by Prof. Vaihinger's 'philosophy of the As If.' And after all, mathematics is entirely composed of fictions, while the use of fictional hypotheses is just now very evident in physics.

Some remarks on freedom on page 238 I found concise to the point of obscurity, and an assertion on page 135 that Euclidean geometry served as the model for Formal Logic seems to need a little qualification. For though Aristotle may have taken the *form* of proof from a (pre-Euclidean) geometry, the *desire* for proof and the demand for its *coerciveness* were rooted in the practice of debate and the needs of dialectics. After all it should not be overlooked that the whole intellectual movement of the fifth century B.C., which led to the invention of the academic life and the evolution of the lawyer, sprang from a practical need. Owing to the growth of democracy the young men of the wealthy classes found themselves at the mercy of democratic juries, with their life and property exposed to the attacks of 'sycophants': as there were no lawyers they could hire, they had to learn to defend themselves with their tongues. The result was the greatest boom in the history of education, paralleled only, if at all, by that now happening in America. Similarly it does not seem to be historically quite correct to derive

the Platonic 'Forms' from mathematics. They seem to have been suggested rather by the need of mediating between the Eleatic One and the Heraclitean Flux, and by the pressure of the predication-problem. Euclid's work was later, and inspired by Platonic metaphysics, which it applied to mathematics.

On the other hand, I have personally been greatly gratified by a large number of undesigned coincidences between Prof. Dewey's contentions and my own. Not only did I welcome his powerful protest against the misrepresentation of the issue raised by Pragmatism as a struggle between 'theory' and 'practice', which I have now been condemning as shallow and fallacious for some twenty years, but I was glad to find that for Prof. Dewey also *data* are really *sumpta* (p. 171), that all 'ideas' are *hypothetical* (p. 158), that *selection* and *risk* pervade all knowing, and that he thinks "the test of consequences more *exact*ing" than that of fixed general rules (p. 265). Finally, however, candour obliges me to confess, as an inference from sundry *obiter dicta*, that Prof. Dewey may be disposed to accord more value to Formal Logic than I am able to do; and as it is rumoured that he is about to write a Logic, I sincerely hope that he will take the opportunity of explaining, very thoroughly, what meaning can be attributed to the notion of 'logical form.' I have now for years been trying to extract an answer to this question from the exponents of Formal Logic, but have never found that what they meant by 'form' was anything more than *verbal*.

F. C. S. SCHILLER.

VI.—NEW BOOKS.

Studies in the Nature of Truth. University of California Publications, vol. xi, 1929. Pp. 232. \$3.00.

THE Philosophical Union of California University is to be congratulated on the appearance of its eleventh volume of 'Publications,' a serious and fruitful enquiry into the nature of truth conducted by eight members of the Union. The importance of the book does not lie in the conclusions reached, for these, in our opinion, are almost invariably unsatisfactory, but rather in the actual argument and in the frequently brilliant suggestions thrown out during its course. (It is not without significance that the two best essayists themselves reveal a certain hesitation in accepting the results of their own reflections.) A noteworthy feature is the extent to which most of the contributors have been influenced by Pragmatism. Before passing to a detailed account of the eight papers, one general criticism, we think, ought to be made. Knowledge and truth, surely, are so closely interrelated that no account of the latter can hope to be complete unless it also makes clear the nature of the former. Yet one feels in reading many of these papers that the writer himself is none too certain as to what he means and does not mean by knowledge. Thus, to take an instance from the opening paper, Prof. Loewenberg talks (on p. 11, and throughout) of "awareness or knowledge" as if they were synonymous terms, and a dozen lines later speaks of himself as "becoming aware, through perceiving or imagining or thinking, of the objects of my belief". Are we to take it that the perceiving itself is a being aware, and if so is it also a knowing? And are we further to identify imagining with knowing? Or are knowing and imagining distinct in spite of the fact that knowing may occur *through* imagining? The paper never enlightens the reader on what is surely a point of the first importance for one who sets out to describe truth. And Prof. Loewenberg is not the only nor the chief offender in this respect.

In spite of the above defect, however, Prof. Loewenberg's paper, entitled "The Prepositional Nature of Truth," setting forward a 'Problematic Realism,' is both able and stimulating. (We are promised a further statement of his views in a work about to be published under the editorship of Prof. George P. Adams on 'Contemporary American Philosophy'.) In what follows we shall attempt to point out the main features of the paper, and we hope that the unavoidable brevity of the summary given will not falsify the argument in any way. Prof. Loewenberg believes that reality exists "prior to mind and hence to knowledge and to truth" (p. 31). But he finds it necessary to qualify his realism and to term it problematic, because the object of our knowledge is not so much the real as it is but that real "forced into alien grooves—the grooves of human belief and discourse and experience" (p. 30), and we cannot be certain that the real actually is what it is compelled to appear to be within these grooves. Hence the problematic character of his realism. But why should we

bother to force the real into 'alien grooves'? The answer, according to Prof. Loewenberg, is that knowledge for us occurs in judgment, and human judgment not only describes something real or existent, but invariably involves three other elements as well. Every judgment involves, firstly, 'the assertion of a dynamic belief,' whose value would be tested pragmatically; secondly, 'a thread of formal discourse,' whose validity is tested by its coherence or consistency; thirdly, 'the expression of an act or state of awareness,' whose criterion is its own immediacy; and, lastly, 'the description of something real,' whose adequacy is determined by its correspondence with the real. If this four-fold analysis of judgment is sound, it follows that we should view the problem of truth from four angles, each of which can be represented by a preposition. Truth, that is to say, is 'multi-prepositional'. "Truth is entertained 'by' minds, it is expressed 'through' assertions, it is immersed 'in' experience, and it is 'about' the real nature of things" (p. 5). Prof. Loewenberg then shows how over-emphasis of one element and neglect of the others has led to one-sided theories of truth, namely, to Pragmatism, to Rationalism, to Mysticism and to certain false versions of Realism respectively. He discusses each of these in turn and shows how they all commit 'the fallacy of the suppressed prepositions,' suppressing three elements in judgment in their eagerness to emphasise the fourth. Prof. Loewenberg's later treatment of the matter may carry greater conviction, but we cannot at present accept his general thesis. We cannot, for instance, see that he has justified the introduction of pragmatic considerations into a theory of truth. Because a knowledge of the difference between truth and falsehood has helped us in life it does not follow that this knowledge exists, even in part, as a consequence of "the necessity for life and for action" (p. 8); nor can we admit that truth is in any way biologically conditioned. And, again, we cannot fully accept as it stands the remaining three-fold division of judgment and of truth. Prof. Loewenberg's effort to blend all four theories into one has, we feel, hardly proved successful. Might it not be held that, from the nature of these theories, any attempt to blend them must inevitably fail?

The contribution of Prof. George P. Adams, entitled 'Truths of Existence and of Meaning,' is also full of interest. The distinction he has in mind between 'existence' and 'meaning' arises from the fact that, according to him, what we *ought* to know as pure theorists is just 'existence,' wholly unmodified in the knowing, but that what we continually *need* to know is the 'value' or the 'meaning,' that which may be of use, that which may serve our purposes. We are not, that is to say, 'disinterested spectators of the real' in knowing. For though the aim of knowledge, as such, is to provide us with 'a faithful register of existence' yet what we actually demand from it is a satisfactory 'plan of action' (p. 40). Prof. Adams defines the term 'meaning' as "the generic term for the kind of thing with which all of our experience, attitudes, and interests other than theoretic and cognitive, have to do" (p. 41). Now truth is the tribunal before which what claims to be knowledge is tested. It pertains not so much to existence as to knowledge. This tribunal is itself ideal and only holds in the realm of 'meanings'. So that knowledge, if it is to be both knowledge and true, faces in two directions. It is "subject to the constraint both of ideal standards and of existence" (p. 50). It therefore follows that true knowledge, in so far as it is true, cannot be 'unalloyed knowledge' of existence. We can only know truly either "existence permeated with meanings" or "meanings alone, divorced from

existence, as in mathematics" (p. 50, surely a very question-begging description of mathematics!). Because our knowledge needs to be true, it cannot be 'purified from the dross of meaning,' it cannot be wholly objective. Consequently, we are faced with two alternatives; on the one hand, an agnosticism that doubts the very possibility of knowledge, and on the other, a pragmatism, in which we no longer seek for knowledge of an independent objective existence, but content ourselves with the useful and the appropriate in the present circumstances. There may exist, however, Prof. Adams suggests, still a third alternative. The mind in its adherence to ideal standards *may* be discovering the very meanings which pertain to real existence. Ideal constructions *may* help to reveal reality as it is, they may be "vehicles through which objective meanings are disclosed" (p. 59). He concludes the paper with the following hypothesis: "that these active operations of the mind provide the one indispensable vehicle through which the inexhaustible wealth of objective meanings native to existence can be known and disclosed" (p. 61). The suggestion, when made, is, we think, a sound one. But why was it kept to the end of the paper? We should have thought it an obvious possibility from the outset. It is not the final hypothesis but the strange use of the word 'meaning' in the body of the paper and the talk about 'existence purified from the dross of meaning' which is most likely to surprise readers on this side of the Atlantic. At the end of his paper Prof. Adams has come to grips with a problem of the first importance: How can a mind carrying on a logically disciplined process of reasoning and using conceptual constructions ever come to know the real? We regret that he did not begin rather than finish with this question, for we feel sure that his treatment of it would have been worthy of attention.

A third interesting contribution of a very different order is that of Prof. Dennes on 'Truth and Perception'. Here again, in spite of the fact that the conclusions arrived at are unsatisfactory, we feel the soundness of much of the argument. Prof. Dennes opens with a careful definition of terms, although unfortunately he leaves undefined the one term which, it seems to us, most needed definition, namely, 'judgment'. He uses the latter term frequently, but never really makes its meaning clear. Beyond telling us that judgments share with acts of perceiving in being 'natural substantial processes' (p. 143), he leaves us in ignorance as to their nature. (How, for instance, are we to distinguish between judging and perceiving?) Truth in judgment, however, he does define. We judge truly when we judge things to be as they are; and the one criterion we have, the one test, is 'perception'. The latter itself is an ambiguous term, as Prof. Dennes points out. It may be used to denote (1) perceiving, (2) the object perceived. By the former may be meant either (1) sense-perception, or (2) 'every kind of immediate awareness,' for instance, the apprehension of the laws of thought. He tells us that he himself will use the term 'perception' to signify 'the concrete compresence of perceiving and perception' and will mean by it every kind of immediate awareness including (but not being confined to) sense-perception. Having made his term clear, he reasserts his belief that the one test of a judgment which claims to be true is such perception, and passes on to show how all other theories ultimately presuppose his. When, however, he does test his judgments by this standard he finds that few of them can be verified. There are only a few facts of which we can be immediately aware. If we admit the existence of 'intuitively evident principles of inference' he will then grant further that "those judgments are true which are validly inferred

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from perceptually verified judgments" (p. 165). But beyond such judgments he finds verifiable and verified truth nowhere. Every statement for which truth is claimed, every hypothesis and theory, must be tested by this standard or norm. No such truth and no such theory can be accepted as true "unless its claims to truth have been perceptually verified" (p. 165). The paper, we feel, is built upon a solid basis, namely, the view that knowing is an immediate act on the mind's part—an act which is its own criterion. The term 'perception' conveys this meaning. But there are very many difficulties in the actual exposition. We have already referred to the ambiguity in the use of the word 'judgment'. Is the knowing present in judging itself a perceiving? If it is we cannot (and do not need to) test such perception by a later act of perception. If the knowing present, however, is not perceiving then what is it? But the greatest difficulty arises from the very wide meaning given to the term 'perception' itself. We are asked to identify such experiences, for instance, as perceiving the sensible world, and 'seeing' that two and two are four, and to hold that both are instances of certain or infallible knowledge. But if we did this we should surely be adopting a very naïve sensationalism, and should be committed to defending a well-nigh indefensible position.

We regret we have no space left in which to consider in detail the remaining five papers. We can, however, recommend them as worthy of consideration. Prof. Lenzen very lucidly sets forth the problem of truth from the point of view of physical science. "In the last analysis," he holds, "the test of the truth of the propositions of physics is correspondence with the data" (p. 109). Prof. Prall provocatively defends the view that truth is wholly 'inaccessible,' that is to say, though we constantly claim to possess absolutely certain knowledge we never can know whether we actually have certainty or not. Instead of certainty we have to be satisfied with a 'grounded faith,' although he admits that the greatest minds do possess a certain 'sense of reality.' Mr. Marhenke, in his carefully-written paper on 'Belief and Fact,' deals largely with the problem of the objective reference of erroneous judgments. "In the case of error nature, so to speak, has actualised a possibility which is not identical with the possibility actualised by a believing mind. Error, therefore, arises as a consequence of the assumption that this identity always exists" (p. 196). Prof. Mackay discusses 'The Nature of Truth in Transformation,' and concludes that truth is "the *measure* of values in a process of inference leading to belief" (p. 134). Knowing, for him, is "a genuinely creative transformation of existence into meaning" (p. 136). Finally, Pragmatism finds a whole-hearted champion in Prof. Mead of Chicago. His contribution, however, is rather obscure, and the argument difficult to follow.

R. I. AARON.

La Pensée Intuitive. I. Au delà du Discours. Par ÉDOUARD LE ROY, Membre de l'Institut, Professeur au Collège de France. Paris: Boivin et Cie, 1929. Pp. vii + 205. Price, 15 fr.

It is difficult to give a satisfactory account of a work of this nature from Part I. alone; for Part II., which is to deal with *Invention et Vérification*, will clear up and supplement much that appears incomplete in the account of intuition in Part I. We must then be content with a few summary indications of the writer's position.

It is M. le Roy's purpose to vindicate for philosophy the method of

intuition, as described by M. Bergson, and to clear intuition of misconceptions, by distinguishing the true intuition from false. His study is careful and discriminating, wise and profound. While he quotes freely from M. Bergson, his mode of treatment is his own, and will appeal to many who are anxious to proceed slowly in these high matters. He speaks as a mathematician, and his work has the qualities of one disciplined in mathematics. He approaches his account from two sides, that of the dynamic schematism of thought, and that of immediacy. Let us consider them separately.

Active thinking, in whatever field, is not (and I should say never can be) a mere external linking of images and conceptions, or of conceptions with one another. Nor can it be completely described in terms of images and conceptions, however complex or manifold. It is a nascent process from which flow images and conceptions with their potential linkages, and which controls the suggestion of these linkages. The phrase "dynamic schema," which recalls Kant's account of schematism, is an admirable one, just because of its closeness of touch with the content to which the activity gives rise. M. le Roy accepts M. Bergson's account of it as "*une représentation simple développable en images ou concepts multiples qu'elle renferme seulement sous forme implicite et potentielle, bref quelque chose d'analogue tout ensemble à un germe et à une impulsion*" (59). He exemplifies a limiting case of it by the familiar instance of our trying to remember a word which is on our lips, but will not come, where we can reject wrong words, but cannot actually find the right one: "*voilà bien, pris sur le vif, le schéma dynamique en activité*" (73). This example, however, must not make us restrict the dynamic schema to cases of such haunting perplexing tension; the schema is operative when the tension is released, and when (as in the case of the solution of some complex problem) the conviction floods over us that we can now go ahead without further obstacles. The dynamic schema is operative throughout the whole process: through the long brooding over the problem, the gradual arising of the tension, the sudden release, the steady forging ahead into the details which the release makes possible. The moment of release of tension in such a case, before the details of the solution have been produced, M. le Roy later in his book describes as the moment of intuition: "*l'intuition porte essentiellement sur un acte, non point sur une chose morte; elle est elle-même un acte, l'acte de pensée créatrice, tel qu'on l'observe par exemple dans la science avant que la tension intérieure de l'invention s'y soit cristallisée en résultat formulable, dans l'art quand la perception n'y est encore que sentiment vécu antérieur à l'image expressive. Elle est chez Racine disant 'ma tragédie est faite' quand pas un vers n'en est encore écrit, ou chez Poincaré contractant soudain l'évidence dynamique d'une découverte. Voilà l'intuition dans sa plénitude*" (152). But it is clear, when we are dealing with a concrete thought-situation, that the conviction that we have the right solution is provisional, and needs to be verified in detail. M. le Roy is clear about this, and it is a part of his thesis. For, as he says, the dynamic schema can only be defined dynamically, by its function of evoking images and concepts. And he goes on: "*Les représentations suscitées font peu à peu un corps au schéma; ce sont les oeuvres de cet esprit, oeuvres où il s'incarne et qui le jugent, parce que leur accomplissement constitue pour lui une véritable épreuve d'expérience réalisante, oeuvres qui par là même le modifient peu à peu, tandis qu'elles en reçoivent l'achèvement de leur détermination. Bref, c'est l'acte vif de pensée créatrice que signifie le schéma dynamique,*

l'acte non encore épuisé, mais au contraire en pleine génération de résultats" (62). True, the operations do not exhaust the act, but nevertheless they are its work, "œuvres où il s'incarne et qui le jugent." *Qui le jugent*: until that judgment has been made, there is no guarantee that the conviction that we have the intuition was a sound one.

It is clear that such images and conceptions, even though they "judge" the dynamic schema, are less than it. However many they may be, they do not exhaust the mind's active experience; it remains eternally richer in potency than they in fact, however much they may be elaborated. If they judge it, none the less it judges them. It is not easy to see in what precise sense one could speak of the dynamic schema as either simple or as possessing unity (though both M. Bergson and M. le Roy do so speak of it), but yet it is clear that it contrasts in some way with the multiplicity to which it gives rise.

Let us come now to the act of intuition from the side of immediacy. M. le Roy describes the possession by the mind of the dynamic schema, in the act of intuition, as the best type of immediacy; and he contrasts it with the discourse to which it gives rise, as a fragmentising of the real, an encrusting of the real with symbols which interpose between the mind and the real, as in short a falsification of the real. Genuine immediacy gives truth and reality, necessity and concrete experience, in an undissolved unity.

Immediacy is to be found at two levels, and it is necessary to distinguish them. Firstly, it appears at the primitive level, before the diversification of the real under the utilitarian impulse of common sense has taken place; and this primitive level it is impossible for us to recover in its purity, though the primitive immediate remains with us throughout, as an element in all our apprehension, however much it may be coloured or distorted by our symbolising in common sense and science. Secondly, the immediate appears at the end of our discursive thinking, in the genuine act of intuition already described, which purifies the primitive immediacy of its symbolic dress, and seizes the real in a direct act. It is clear here that the genuine immediacy, like the dynamic schema, is the result of a long process of discursive analysis, and is to be described as pure activity rather than as passivity. M. le Roy's best illustration of such an immediacy, and of the fact that it is a result and not a beginning, is the *Cogito* of Descartes, reached as it is after a process of doubt, of reflective analysis, removal of confusions, resulting in an act in which being and knowing, fact and certainty, interpenetrate.

This account of immediacy, however, seems to be somewhat at strife with the account of the dynamic schema. According to the former, we should seem to be most closely in touch with the real at the moment before our act of intuition becomes prolonged into the discursive ordering of images and concepts; according to the latter, the sole function of the dynamic schema is to produce this discursive ordering: even though it controls and criticises the discursive ordering, it is bodied forth in it, tested by it, and until so embodied, has an element of precariousness about it.

Further, the doctrine of immediacy seems to be responsible for a restriction of intuition which the facts of the nature of thinking do not seem to bear out. Intuitive thinking is identical with metaphysical thinking, M. le Roy says; this remark seems clearly to be due to his view of ordinary and scientific thought as discursive and as falsifying. But the account of the dynamic schema applies to all thinking whatever (in spite of M. le

Roy's occasional limitation of it), whether in mathematics, or in a problem in chess, or in what you will. The basis of all active thinking is an active intuitive grasp of the situation in a dynamic schema: if so, then M. le Roy's statement would involve that all thinking is metaphysical.

My general difficulty then is, firstly, that the account of intuition in terms of dynamic schematism fails to restrict intuitive thinking to metaphysics, and so fails to distinguish between science and philosophy; and, secondly, that the account of intuition in terms of immediacy involves that the prolongation of the act of intuition into a discursive ordering of images and concepts is a degradation, a soiling of the purity of the act, while its account in terms of the dynamic schema involves that this prolongation is rather a fulfilment, a completing, a perfecting of the act.

It is possible that I have misunderstood, and that Part II., which will deal with the return from the *au delà du discours* to the concrete, will make the situation clearer. The source of the difficulty seems to lie, firstly, with the view that common sense and science, in dealing with experience from particular (and especially from practical) points of view, are falsifications, and secondly, with the view that so long as our thinking is pure activity, it is incapable of falsification. It seems, however, better to await the appearance of Part II. than to indulge in criticism which may be beside the point. It is clear from Part I. that the whole work will be of great interest and importance.

L. J. RUSSELL.

Les Puissances de l'Abstraction. By FRÉDÉRIC PAULHAN. Bibliothèque des Idées. Paris: Librairie Gallimard, 1928. Pp. 313. Fr. 30.

M. PAULHAN first selects for lengthy description six facts representative of the mental phenomena, the mechanism of which he intends investigating. His object is to show that the explicative principles worked out in his *l'Activité mentale et les Éléments de l'Esprit* are also sufficient to account for these phenomena of "abstraction."

The data are quite normal experiences; five of them arose while the author was reading. (i) In two cases, there comes to mind a word which has no apparent connexion with the words read. (ii) While proof-reading he has the impression of having passed over some typographical error, turns back but does not find it, reads *on* and does. (iii) He transposes in memory an adjective from its given substantive in the text to another which, though in the text, is not there qualified by it. (iv) A similar case in which what is transposed is an italicised word. (v) The principal datum is a detailed record of an attempt, lasting four days, to complete recall of a certain stanza, a fragment of which (with omissions and partly inaccurate) had come to mind. The first chapter is restricted to a full and careful description of these data, in Chapters II.-VII. their explanation is undertaken.

What is the character of the group of processes occurrent on each of these occasions? M. Paulhan tries to give a generalised answer, showing by what psychological mechanism ideas, desires and tendencies persist, and from this, how mind develops and changes. He first postulates that any conscious state continues existing until "opposed." And this postulate is soon extended to apply also to mental activities and mental "factors" that are not conscious states nor consciously existent. We find on inspection that a conscious state is scarcely instated before it "decomposes" and yields to a successor. Some of the "factors" of the state persist,

however, and, in a way not explained, connect up with ideas and tendencies already persisting in that mind. So parts of the original experience continue to exist while no longer parts of any conscious state. Such dissociation is a general property of all mental states, abstraction is one of its specific determinations. The dissociated factor persists as a "mental attitude," as a "power" which tends to pass into an activity. So M. Paulhan seems to hold that dissociated factors have two characteristic activities, one by which they are actively persistent and potentially prehensive, another by which they reinstate themselves in consciousness, by connecting with other persistent factors, or with some perceptual factors, so as to constitute together an instance of, say, remembering or recognition. M. Paulhan repeatedly emphasises this active rôle fulfilled by surviving factors of past conscious states. Such factors tend to reinstall, not only themselves, but the states to which they originally and severally belonged. Failing this, they connect themselves with certain other states that were momentarily connected with their original states.

This introduces a new explanatory concept—that of the *système préhenseur*. This phrase is presumably meant to describe that unity of properties which is a residue of some former experience. Such *systèmes préhenseurs* too are essentially persistent and active, ever trying to complete their present incompleteness. They are described as being ever ready to "seize upon" and connect up with any factor in a present act (of, say, perception or memory) that can aid them in re-establishing their original completeness. But *systèmes préhenseurs* never can, in fact, complete themselves. At best a fuller, though still partial, reinstatement occurs (cf. the memory-image), and sometimes the reinstatement is so fragmentary as to be almost without significance. Now, whenever reinstatement is incomplete, "substitution" occurs also. The function of those factors of the original state that are impervious to recall is now carried out by new factors similar to them. The similarity of these substitutive factors, however, only enables them to perform a function *analogous* to that performed by the original factors, and the degree of analogy varies in different instances. It is then by such substitution of "new" factors on the part of a *système préhenseur* that the element of novelty comes to characterise our experiences,—that "innovation" and "invention" arise. (Apparently a factor is "new" when (i) it is reinstated, but (ii) by a *système préhenseur* other than that which is a residue of the original conscious state to which the factor itself belonged.) The mechanism of recall and that of invention or innovation are at bottom identical. The diversity and specificity of mental functions are throughout made to depend on the persistent, active and connective character of dissociated factors of past experiences. Hence, the formation and transformation of our ideas, sentiments, conduct—of our minds themselves, result from causes which reduce to two: (i) the persistence and activity of abstracted factors, (ii) the *avidité des systèmes préhenseurs incomplets*. And in the last two chapters M. Paulhan tries to show that the nature of personality, habit, sentiment, and social conduct generally, can be so explained.

Seeing that these reinstatements, although concrete experiential unities, are effects of a fundamentally abstractive process, one is not surprised to find M. Paulhan's account of the process itself even more, and highly, abstract. His book is in consequence not easy reading, despite the lucidity of his French. But constant reference back, by way of illustration, to one or other of his six facts, lends a concreteness to his explanations that is bound to be absent from a plain statement of them in a review.

M. Paulhan admits there is a certain amount of hypothetical construction in his explanations, but thinks it at least pragmatically justified. His interpretation of mind is non-atomistic, based rather on a biological principle of functional activity, and his problems are essentially genetic ones. But, *toute réserve faite*, the explanatory part suffers from two sorts of defect. The numerous explanatory concepts introduced seem to offer some good 'shaving' for Occam's razor. And they are seldom defined, and can often be only vaguely understood. "Mental activity" assumes such protean forms that one cannot be sure whether it is one, or a plurality of activities that is postulated for each mind. For instance (p. 200), no account is given of the connexion between "personal activity" which systematises the mind into an altering unity, and the (apparently other and particular) acts, e.g. of dissociation, combination, association, substitution, perception and representation. The free use of "factor" which applies now to characteristics of percepts, now to "tendencies" and "forces," is confusing. The assumption of such factors existing, being active, tending to connect, to reinstate, and yet remaining unconscious over periods, raises difficulties additional to those already present in an ordinary hypothesis of traces. And the notion of "forces" is even less clear. A psychical state is defined (pp. 39-40) as "a bundle of psychological facts, *that is*, of systems of relations and qualities and forces, associated into groups that are more or less distinct, though united by coming together in one personality." Again, "pre-existent forces" and "undifferentiated psycho-organic forces" are introduced (p. 106, p. 143) without definition. The diffuseness of the terms themselves and the free usages made of them lead one to feel that, until given greater precision, they will rather conceal than reveal the scientific worth of M. Paulhan's theory. M. Paulhan has however worked over his material in a way that is often extremely suggestive. This suggestiveness and the excellence of his phenomenological descriptions form the chief merits of his book.

S. V. KEELING.

Immanuel Kant's Critique of Pure Reason. Translated by NORMAN KEMP SMITH. London: Macmillan & Co., Ltd., 1929. Pp. xiii + 681. 25s. nett.

PROF. KEMP SMITH's translation of the *Critique of Pure Reason* has long been awaited by teachers of philosophy, and it may be said at the outset that it fulfils the expectations which have been entertained in regard to it. In spite of Prof. Smith's too kindly eulogy of his predecessors in the work of translation, it is somewhat of a scandal that the English-speaking race has had to wait nearly a century and a half for a scholarly translation of Kant's greatest work. That outstanding blot upon our philosophical scholarship has now been removed, and all who are interested in the study and the teaching of Kant must express their gratitude to Prof. Smith, and their congratulations on the successful completion of his long and laborious task.

The two demands made of a philosophical translation are that it should be firstly readable and secondly accurate; and the new translation satisfies both these demands. The difficulties involved in attaining these ideals are very great when dealing with so complicated a work as the *Critique*; and indeed a translation which is accurate is likely to be unreadable, while one which is readable is likely to be inaccurate. No translation of Kant can be easy reading, but Prof. Smith's method of dividing up Kant's

long sentences, while it loses something of Kant's subtlety and solidity, has resulted in a work which reads like English, and carries on the reader from stage to stage in the argument.

It is an immense advantage to have a translation which for the first time in English gives references to the pages of both the first and the second edition of the *Critique*. The English reader can now follow the relation between the two editions, and Prof. Smith is to be congratulated on the innovation which places the second edition version of the Transcendental Deduction and the Paralogisms immediately after the first. The task of the translator has been facilitated by the edition of Dr. Raymond Schmidt, published in 1926, an edition which, as Prof. Kemp Smith justly remarks, is 'quite invaluable'. The relation of the two editions to one another might have been made still clearer, if Prof. Smith had followed Dr. Schmidt's example in printing all passages peculiar to either edition in italic type.

At the close of the introduction Prof. Smith expresses the fear that there may still be major and minor errors undetected in the text. A careful scrutiny of several important passages by the present reviewer has brought to light no major errors, and indeed no definite errors at all, although there are naturally passages in which other interpretations might be regarded as more satisfactory by some students of Kant. Prof. Smith, however, sticks very closely to the German text, and his work has the great merit of not twisting Kant's words in the interests of any pre-conceived theory.

Some remarks may be added on points of minor importance.

One of the difficulties in dealing with Kant lies in his technical terms, and on these there are bound to be differences of opinion. Personally, I am very glad that Prof. Smith has translated *Anschauung* throughout as 'intuition' and not as 'perception'. I do not feel quite so happy about the use of 'representation' for *Vorstellung*, but on this point perhaps only those who have attempted to translate considerable parts of the *Critique* are entitled to an opinion. The word 'representation' has certainly a long tradition behind it, and it has the very great advantage that there is also a corresponding verb, 'to represent'. On the other hand, *Vorstellung* is a 'presentation' rather than a 'representation'; it has a less technical atmosphere in German than 'representation' has in English; and the continual use of an unusual word like 'representation' in place of the ordinary word 'idea' is apt to give in English an appearance of intolerable heaviness. That Prof. Smith has on the whole avoided such an appearance is perhaps the best justification of his choice.

Another word which gives difficulty is the word *Erkenntnis* with its plural *Erkenntnisse*. If we translate *Erkenntnis* as 'knowledge,' what are we to make of the plural? Are we to speak of 'knowledges' or of 'knowings' or of 'cognitions'? All of these present difficulties, and Prof. Smith has sought to avoid these difficulties by speaking of 'modes of knowledge'. This seems to me unfortunate, for the word 'mode' had in Kant's time a very definite technical meaning, and whatever else *Erkenntnisse* may be, they are certainly not 'modes' of knowledge. It would have been better to speak of 'acts' or 'instances' of knowledge, or even to accept the word 'cognitions' in spite of the objections which may be raised.

One or two small points may be noted. In B. xii 'volume' is surely a misprint for 'column'. In A. 99 *geradezu* should be translated; it corresponds to *unmittelbar* in A. 120. In A. 119 is it not a little perverse

even for a Scotsman to translate *wir wollen* as 'we shall'? In A. 142 should not *gebracht* be translated as 'brought into' rather than as 'reduced to'? And in A. 164 (note by translator) is it correct to say that *nicht allgemein* means 'specific,' and not, as Kant himself says, 'singular'?

These, however, are mere trifles. Along with them may be noted one passage where, as so often in Kant, the wording is ambiguous, and where perhaps another version is more plausible. I do not speak with confidence, but I think I could give reasons in A. 101 for holding that 'such as a thorough-going synthesis of reproduction renders possible' is preferable to 'such as renders a thorough-going synthesis of reproduction possible'.

One last point. In places Prof. Smith seems to me too ready to accept emendations where they are unnecessary. To give one example—the emendation of Adickes in A. 103-104. Kant's own text may be translated as follows: 'This consciousness may often be only faint, so that we connect it with the production of the idea only in the outcome, and not in the act itself (that is, immediately)'. This seems to me to be a precise statement of what Kant means, and has parallels in other passages. An idea which is *schwach*, 'faint,' or *dunkel*, 'obscure,' is one which is not known immediately, but mediately, known in this case not in the act itself, but in the outcome or the result. There is a passage in the *Anthropologie*, § 5, which throws light both on this sentence, and on the corresponding statement in A. 117 n.

These criticisms are manifestly concerned only with matters where differences of opinion are possible, and perhaps inevitable. The solid fact remains that English Kantian scholars have now a translation to which they can always turn for light on difficult passages; and that beginners in philosophy, who too often are lacking in knowledge of the German language, can now read Kant in a version which gives them, in an English dress, an accurate and readable statement of the doctrines of the great master.

H. J. PATON.

Ethical Problems: An Introduction to Ethics for Hospital Nurses and Social Workers. By BEATRICE EDGELL, D.Litt., Ph.D. London: Methuen & Co., Ltd., 1929. Pp. ix + 149. 5s.

THIS work is a successful attempt to apply philosophical knowledge. Prof. Edgell sets out to deepen the interest of hospital nurse and social worker in their own particular vocations. To secure this end they are introduced to the ethical (and psychological) teaching of the day. A brief résumé of the argument will show how this is done.

The work is divided into two parts, the first setting forward the principles, and the second their applications. In the introduction it is made clear that the author considers ethics to be a normative enquiry into three problems, namely, those of the reference, the meaning and the determination of the moral judgment respectively. As a preamble, a sketch is given of the main development of human behaviour. Dr. Edgell begins with native reflexes—avoiding, rightly in our opinion, all reference to 'tropistic' responses in dealing with human behaviour—and, passing to the so-called 'conditioned reflex,' which she regards as a misnomer, shows how the latter already involves a sense of values. The presence of a sense of values becomes increasingly clear in instinctive and impulsive behaviour. Where the valuable is not immediately realisable there ensues

a state of desire followed by intentional action, the sphere of the moral judgment proper. (Dr. Edgell's example, pp. 23-24, gives, in our opinion, too narrow a meaning to desire. Surely, if the action is at all intentional and not merely impulsive an element of desire must be present.) In the second chapter the reference and meaning of moral judgments are considered. Our concern should be with the intentional act. Dr. Edgell tends to believe that the moral judgment should refer both to the motive and to the consequences of the act, while the whole character of the doer is also within its reference. The three must be taken together. The meaning of the moral judgment is next considered, and, here, after referring to certain plausible but erroneous views, it is pointed out that the moral judgment, firstly, issues an imperative, secondly, sets forward a standard, and, thirdly, attributes a quality 'good' (or 'bad') to conduct. The third chapter sets forth the principles according to which we determine the 'ought,' the 'right' and the 'good'. The traditional theories are briefly examined, firstly, those which concern themselves with moral law, emphasising the 'ought' and the 'right' in the moral judgment, and, secondly, those which concern themselves with the moral end, emphasising the 'good'. An effort is also made to relate the two groups of theories.

The second part of the book applies the principles set forth in the first. There is again a psychological preamble to consider the meaning of self, sentiment, character, and free will. (We thought Dr. Edgell's argument on page 78 against the over-emphasis of the sentiment of self-love by modern psychologists both neat and timely.) Actual ethical problems which frequently meet nurse and social worker in the discharge of their duties are then considered in detail; and in a final chapter the type of character essential for both of these vocations is set forth.

Dr. Edgell is to be congratulated on doing so much in so little space, and on her success in introducing the reader to all the more important ethical theories without turning the book into a mere catalogue. The exposition is lucid and accurate, though we do not feel quite sure that what is said of the Kantian position on page 61 is altogether sound. Prof. Edgell, however, is here dealing with the very difficult problem of the relations between end and law. We think that this part of the argument might have been expanded with advantage. It is impossible to deal with the problem fairly in one or two paragraphs.

The work throughout is characterised by a maturity and a robustness that make it well worth reading. We hope it will find many readers amongst those for whom it was written. Finally, we can see no reason why it should be confined to nurses and social workers though meant primarily for them. It can safely be recommended to any student beginning to read ethics.

R. I. AARON.

Moral Law and the Highest Good. By E. MORRIS MILLER, M.A., Litt.D., Professor of Psychology and Philosophy in the University of Tasmania. Macmillan & Co. (Melbourne University Press), 1928. Pp. xii, 235. Price 6s. 6d.

THE author deals primarily with the *Critique of Practical Reason*. The chief defects which he finds in the earlier writers upon Kant's ethical theory are that their accounts of Kant's views are too incidental, either in the sense that Kant's doctrines are referred to chiefly in contrast to the writers' own, or in the sense that those writers treat the *Critique of Practical Reason* as quite subordinate to the *Critique of Pure Reason*. Now it is a frequent

criticism that the *Critique of Practical Reason* falls into two contradictory parts—the *Analytic* developing a theory of duty as obedience to the Moral Law as against the desire for Happiness; the *Dialectic* seeking, by artificial argument, to give Happiness once more a respectable status in the moral life. Prof. Miller's object is to show that, whatever be the adequacy of Kant's interpretation of moral experience in general, this 'inconsistency' has been very much over-emphasised; and in support of his view he gives a fairly exhaustive account of the contents of the Second *Critique*. The volume is primarily expository. The author does indeed say that he offers a "constructive exposition and criticism," but one's main impression is that criticism is largely subordinated and confined within fairly narrow limits.

So far as Prof. Miller's primary object is concerned, he has in a great measure succeeded in attaining it; and, when there is a natural temptation to acclaim or condemn as the 'true Kantian doctrine' what is after all but one side of his thought, it is wholesome and salutary to be reminded of what the *Critique of Practical Reason* does actually contain, and that it is a many-sided and possibly very unequal piece of work.

We were to confine our attention to the main object of the volume under review, perhaps we have said all that requires saying. The reader will not (I imagine) charge Prof. Miller's book with any serious omissions or one-sidedness. But it may be permissible to raise the question as to the ultimate philosophical value of a work of this kind. Prof. Miller is surely working on sound lines in approaching Kant's ethical writings from the point of view that they are not merely corollaries deduced from the doctrines of the *Critique of Pure Reason*; because, despite the fact that Kant's Moral Philosophy is very much hampered by his fondness for the terminology of the First *Critique*, it is impossible to believe that his ethical doctrines were not based primarily on a study of the "facts of moral experience." But it is also surely the case that the study of the *Critique of Practical Reason* must be approached from either of two angles: Either (first) by undertaking an independent analysis of moral experience in order to understand what were the living facts Kant stressed and claimed to interpret; and to do this it is essential that we should begin with ordinary speech and the ordinary moral consciousness, and not by pitching headlong into characteristically Kantian terminology. Or (secondly) if we believe that Kant's moral theory cannot be expounded in terms other than his own, we must begin with a critical examination of the *Critique of Pure Reason*, and of the doctrines in which that terminology is rooted. It forms (I believe) a serious limitation to the value of his work that Prof. Miller has not followed either of these plans; and with regard to the question of the permanent significance of Kant's ethics, one feels inclined to say that Prof. Miller has left us precisely where we were. The chief value of his work is not that it throws much new light on Kant's mind, but that it gives a pretty balanced presentation of the theory as a whole.

W. D. LAMONT.

Geschichte der philosophischen Ideen. By H. K. SCHJELDERUP, Professor and Director of the Psychological Institute, University of Oslo. Berlin: Walter de Gruyter, 1929. Pp. viii, 232. M. 7, bound M. 8.

THIS book deserves the wide recognition it already enjoys. In its original tongue, Norwegian, it has passed into a second edition, from which the present German translation has been made, and it has been honoured

with recommendation by the League of Nations. It exemplifies afresh the easy clarity of the author's earlier book, *Psychologie*, reviewed in *MIND*, July, 1929. As a lucid expositor he is comparable with Russell in our own country, and the versatile and fertile Messer in Germany.

The book is, however, too elementary to attract attention here, since, unfortunately, few of our beginners read German. It is a first survey of modern philosophy from Nicholas of Cusa to Husserl. There are peculiarities of proportion: for example, Hegel has four pages, as few as Mill; but probably the author felt that the only alternative to a very brief exposition of so difficult a thinker would be a very long one. The account of each philosopher is orthodox but fresh, and always extremely discriminating. Where the author's own sympathies lie I cannot gather; throughout he expounds his subject, not himself. He is free from both helpless prepossessions and vanity.

Both the translator and the publisher claim that the distinctive feature and justification of the work is that it brings out "the psychological in the philosophical experience of the various thinkers". This is simply a fine phrase, another instance of the banal use of "psychological". All I can find is that general attempt to understand a thinker's equipment and intentions which appears, and must appear, in every history of philosophy. The distinction of the work lies entirely in its selectiveness and remarkable clarity. It contains but one extravagance, where the author approvingly quotes Falckenberg: "The Greek world-view is as classic as the sculpture of Phidias and the epic of Homer, the Christian one as eternally valid as the architecture of the Middle Ages, and the modern one as irrefutable as the poetry of Goethe and the music of Beethoven". The analogy with art does not hold; different works of art may be equally beautiful, but incompatible world-views cannot be equally true. It would have been better, especially in a book for beginners, to say quite simply that bygone philosophic theories, unlike bygone scientific ones, may still be possible solutions of our problems, and therefore deserve still to be studied.

T. E. JESSOP.

The Psychology of the Infant. By SIEGFRIED BERNFELD. Translated by ROSETTA HARWITZ. London: Kegan Paul, 1929. Pp. xi + 309. 15s.

THIS book offers a study of the mental development of an infant from birth to the normal time of weaning. The author adopts the Freudian point of view and seeks to link this up with the general doctrines of child psychology and with the observations of infant behaviour. Unfortunately, Dr. Bernfeld has had to rely entirely upon reports of others for such observations, and we cannot help thinking that his treatment of the subject might have been greatly modified if he had had the advantage of day to day observation of several children during the whole period under consideration, and later. In some matters, dependent on such observations, he seems to me to go seriously wrong, as for example in his minimising of the importance of the method of trial and error in the infant's learning of such movements as those involved in bringing an object up to the eyes for inspection.

The book is divided into five sections: (A) The New Born (in which such functions as sleep, crying, nursing and reflexes are treated); (B) First Progress (including a discussion of seeing, hearing, "the oral zone," instinct groups, perceptions, etc.); (C) The Instinct of Mastery (dealing

with hand movements, crawling, the "instinctual components of mastery" and "libido development of the infant at the grasping age"); (D) Trauma and Frustrations (treating "birth as a trauma" and "the weaning period"); and finally, (E) "The Infant and its World" (including "the affective attitudes," "perception and instinct," and the significance of weaning).

Whatever one may think as to the validity of Dr. Bernfeld's fundamental assumptions and the viewpoint from which he starts, it is undoubtedly useful to have an attempt to link these up with the study of infancy, and many of his observations on individual matters are most suggestive. It cannot be said, however, that he is very lucid or convincing in the development of his main thesis, though it is possible that this is partly due to the translation.

C. W. V.

Über Sinn und Sinngebilde, Verstehen und Erklären. By HEINRICH GOMPERZ. J. C. B. Mohr (Paul Siebeck), Tübingen, 1929. Pp. 256. M. 12.50, bound M. 15.

THIS book is an able and valuable contribution to the discussion of certain philosophical topics, and may be recommended to anyone who is interested in the problems discussed, especially as it possesses the merit of clearness. Its content is very relevant to the aims and ideals of the sciences or of knowledge in general, and is of special interest at the present day when most thinkers feel both that science tends to be merely descriptive, and yet that this does not satisfy the demands of the scientific ideal.

The book starts by analysing carefully the various senses in which something may be said to have meaning (Sinn). In the case of language meaning is based on habit and convention, but the author is principally concerned with the wider question as to what we have in view when we inquire into the significance (meaning) of an element or event in the world or even ask whether the world as a whole has a meaning. He analyses what may be meant by these questions without developing his philosophy so far as to provide an answer. Central but not exclusive is the conception of purpose, *e.g.*, a law or institution is senseless (without meaning) if it thwarts its own purpose, and in general he suggests that "meaningfulness" implies both that there is a demand on us to adopt a certain attitude of mind and that this attitude is practicable. Nonsense arises when the attitude is demanded and yet proves impossible, *e.g.*, a senseless or self-contradictory sentence, since it presents itself as speech, calls upon us to grasp its contents intellectually and then completely thwarts our attempt to do so.

For further development of the theme the following distinctions are fundamental: (1) the distinction between the three realms—physical, psychical and "geistig." The last includes whatever belongs neither to individual minds as such nor to the physical world, and therefore embraces alike social institutions, truths of reason, such as those of pure mathematics, concepts of all kinds, and values. The conception of this third class of being is now very familiar in German philosophy. The author shows that as given in human life the three realms are inextricably mixed, and occur together as different sides of almost any event, but that the sciences try to separate them; (2) the distinction between understanding (*verstehen*) and explaining (*erklären*). The latter is the object of the physical sciences, and depends on bringing the particular case under a general law; it is indispensable if we are to obtain definite knowledge,

but does not necessarily involve our seeing the laws to be intelligible. Opposed to this is a kind of sympathetic understanding by which we grasp the meaning (Sinn) of an event or concept, *i.e.*, see it as an intelligible part of a wider scheme. In general, though with reservations, it is said to involve a reference to purpose, as meaning itself has been shown to do. But there is a second line of thought in the book which I find hard to reconcile with the one which refers it to purpose, for this second view suggests that it is merely the result of familiarity. It is to be regretted that the author does not make it clear how he reconciles the two standpoints: *e.g.*, is the first a genuine and the second an illusory "understanding," or can one in any way be reduced to the other? He thinks that "understanding" is primarily directed towards the individual case, not the class, and that it is the only method in psychology, a view which seems hardly tenable in face of the recent development of the science. But in general it remains for him inferior to explanation. It is indeed far more capable of providing a kind of intellectual satisfaction; but for the discovery of new truths it is only "an inadequate prelude, a provisional substitute" for explanation. This conclusion seems to me unsatisfactory, because the author does not distinguish between genuine and spurious "understanding." Surely to see why one truth or (if possible) one event must follow from others instead of merely knowing as a fact that it does so is always a higher ideal of knowledge, though in many cases it may be quite unattainable and though in some the attempt to achieve it has led men into absurdities.

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Received also:—

- G. Santayana, *The Realm of Matter*, London, Constable & Co., Ltd., 1930, pp. xv + 209, 12s.
- H. Wildon Carr, *Cogitans Cogitata*, London, Favil Press, 1930, pp. xii + 110, 6s.
- G. P. Adams and W. P. Montagne, ed. by, *Contemporary American Philosophy: Personal Statements*, Vols. I and II, London, G. Allen & Unwin, 1930, pp. 450; 447, 16s. each.
- G. Marcel, *Journal Métaphysique*, 2nd ed., Paris, Librairie Gallimard, 1927, pp. xi + 342, 35 fr.
- A. Masnovo, *Problemi di Metafisica e di Criteriologia*, Milan, "Vita e Pensiero," 1930, pp. vii + 50, L. 5.
- R. Damien, *Le Monde Intérieur*, Paris, F. Alcan, 1930, pp. 141, 20 fr.
- G. Tarozzi, *L'Esistenza e l'Anima*, Bari, G. Laterza e Figli, 1930, pp. xvi + 240, L. 15.
- É. Meyerson, *Identität und Wirklichkeit*, trans. into German by K. Grelling, Leipzig, Akademische Verlagsgesellschaft, 1930, pp. xl + 534, M. 21.
- M. C. Swabey, *Logic and Nature*, New York University Press, pp. xiv + 384.
- Sir O. Lodge, *Beyond Physics*, London, G. Allen & Unwin, Ltd., 1930, pp. 172, 5s.
- C. E. Hooper, *The Fallacies of Fatalism*, London, Watts & Co., 1930, pp. xi + 211, 10s. 6d.
- E. Rignano, *The Nature of Life*, London, Kegan Paul, 1930, pp. x + 168, 7s. 6d.
- E. R. Rost, *The Nature of Consciousness*, London, Williams & Norgate, Ltd., 1930, pp. 158, 12s. 6d.
- A. Wolf, *Textbook of Logic*, London, G. Allen & Unwin, Ltd., 1930, pp. 407, 10s.

- F. Kaufmann, *Das Unendliche in der Mathematik und seine Ausschaltung*, Leipzig, F. Deuticke, 1930, pp. x + 203, M. 12.
- H. F. Standing, *Spirit in Evolution*, London, G. Allen & Unwin, Ltd., 1930, pp. 312, 10s. 6d.
- H. M. Foston, *Man and the Image of God*, London, Macmillan & Co., Ltd., 1930, pp. 228, 7s. 6d.
- G. Galloway, *Religion and the Transcendent*, University of London Press, Ltd., 1930, pp. viii + 154, 4s. 6d.
- C. Humphreys, *A Religion for Modern Youth*, London, Anglo-American Publications, 1930, pp. 31, 1s.
- Ll. Powys, *The Pathetic Fallacy: A Study of Christianity*, London, Longmans, Green & Co., 1930, pp. vii + 129, 5s.
- H. O. Eaton, *The Austrian Philosophy of Values*, Norman, University of Oklahoma Press, 1930, pp. 380, \$5.
- H. Driesch, *Ethical Principles in Theory and Practice*, trans. by W. H. Johnson, London, G. Allen & Unwin, Ltd., 1930, pp. 248, 7s. 6d.
- A. Autin, *Laïcité et Liberté de Conscience*, Paris, F. Alcan, 1930, pp. 214, 15 fr.
- G. Williams, *The Human Perspective: Being an Interest Theory of Value*, Oregon, E. F. Kramer, pp. 63.
- P. Servin, *Les Rythmes comme Introduction Physique à l'Esthétique*, Paris, Boivin & Cie., 1930, pp. 208, 12 fr.
- C. Camichel, *Leçons sur les Conduites*, Paris, Gauthier-Villars & Cie., 1930, pp. 101, 30 fr.
- J. Pérès, *Les Sciences Exactes*, Paris, E. de Boccard, 1930, pp. 196, 25 fr.
- H. Metzger, *La Chimie*, Paris, E. de Boccard, 1930, pp. 169, 20 fr.
- L. Ambard, *La Biologie*, Paris, E. de Boccard, 1930, pp. 113, 25 fr.
- F. Jodl, *Geschichte der Ethik, Bd. I.: Bis zum Schlusse des Zeitalters der Aufklärung*, 4th improved ed., Stuttgart, J. G. Cotta'sche Buchhandlung Nachfolger, 1930, pp. xii + 687, M. 20.
- V. D. Macchioro, *From Orpheus to Paul: A History of Orphism*, New York, H. Holt & Co., 1930, pp. 262, \$3.
- A. Levi, *Le Idee Religiose di Euripide e la sua Visione della Vita (Reale Istituto Lombardo di Scienze e Lettere, LXII, 16-20, 1929)*, Milan, U. Hoepli, 1930, pp. 909-919.
- Sir J. G. Frazer, *The Growth of Plato's Ideal Theory*, London, Macmillan & Co., Ltd., 1930, pp. xi + 114, 7s. 6d.
- P. Frutiger, *Les Mythes de Platon*, Paris, F. Alcan, 1930, pp. 295, 35 fr.
- A. Levi, *Il Problema dell'Errore nella Filosofia Greca prima di Platone (Athenæum, VIII, 1 Jan., 1930)*, Pavia, Amministrazione dell'Athenæum, pp. 18.
- J. Albo, *Sefer Ha-Ikkarim*, Vol. IV, Parts 1 and 2, edited by J. Husik, Philadelphia, Jewish Publication Society, 1930, pp. 272, 273-597.
- Opera hactenus inedita Rogeri Baconi, Fasc. X, Questiones supra libros primæ philosophiæ Aristotelis*, edited by R. Steele and F. M. Delorme, Oxford, Clarendon Press, 1930, pp. xxxii + 360.
- A. Masnovo, *Da Guglielmo d'Auvergne a San Tomaso d'Aquino, Vol. I: Guglielmo d'Auvergne e l'Ascesa verso Dio*, Milan, "Vita e Pensiero," 1930, pp. viii + 283, L. 20.
- P. Fr. H. Buonpensiere, O. Fr. P., *Commentaria in I. P. Summæ Theologiæ S. Thomæ Aquinatis a Q. XXVII ad Q. XLIII, Vergaræ*, "El Santissimo Rosario," 1930, pp. viii + 608.
- B. Romeyer, *Saint Thomas et notre connaissance de l'esprit humain (Archives de Philosophie, Vol. VI, Cahier II)*, Paris, G. Beauchesne, 1928, pp. 114, 18 fr.

- E. Hoffmann, *Das Universum des Nikolaus von Cues*, Heidelberg, C. Winter, 1930, pp. 45, M. 2.50.
- J. Böhme, *De Electione Gratiae and Quaestiones Theosophicae*, trans. by J. R. Earle, London, Constable & Co., Ltd., 1930, pp. lxx + 327, 10s. 6d.
- G. della Volpe, *Il Misticismo Speculativo di Maestro Eckhart*, Bologna, L. Cappelli, 1930, pp. vii + 291, L. 20.
- M. d'Asbeck, *La Mystique de Ruysbroeck l'Admirable*, Paris, E. Leroux, 1930, pp. ix + 309.
- Pico della Mirandola, *On the Imagination*, Latin Text with Introduction, Translation and Notes by H. Caplan, New Haven, Yale University Press (London, H. Milford), 1930, pp. 102, 4s. 6d.
- G. Sortais, *Le Cartésianisme chez les Jésuites Français au XVII^e et au XVIII^e siècle*, Paris, G. Beauchesne, 1929, pp. vi + 109.
- S. H. Mellone, *The Dawn of Modern Thought: Descartes, Spinoza, Leibniz*, London, H. Milford, 1930, pp. 124, 4s. 6d.
- B. Landry, *Hobbes*, Paris, F. Alcan, 1930, pp. 278, 40 fr.
- A. Ibsen, *John Locke's Lære om Almenideer*, Copenhagen, Jespersen & Pios, 1930, pp. 49, Kr. 1.50.
- É. Bréhier, *Histoire de la Philosophie: Tom II, 2, Le XVIII^e siècle*, Paris, F. Alcan, 1930, pp. 318-576, 20 fr.
- B. Bolzano's *Schriften, Band I: Functionenlehre*, Prague, Königliche Böhmische Gesellschaft der Wissenschaften, 1930, pp. xx + 183 + 24.
- H. Fels, *Bernard Bolzano*, Leipzig, F. Meiner, 1929, pp. x + 109, M. 5.
- M. Gueroult, *L'Évolution et la Structure de la Doctrine de la Science chez Fichte*, 2 vols., Paris, "Les Belles Lettres," 1930, pp. 383, 252, 40 fr. each.
- G. Lasson, *Einführung in Hegels Religionsphilosophie*, Leipzig, F. Meiner, 1930, pp. 150, M. 8.
- G. W. F. Hegel, *Vorlesungen über die Beweise vom Dasein Gottes*, ed. by G. Lasson, Leipzig, F. Meiner, 1930, pp. xi + 187, M. 9.
- M. R. Stoll, *Whewell's Philosophy of Induction*, Lancaster, Pa., Lancaster Press, 1929, pp. iii + 125.
- J. Pommier, *Deux Etudes sur Jouffroy et sur temps*, Paris, F. Alcan, 1930, pp. vii + 74.
- Archives de Philosophie, Vol. VI, Cahier IV: Bibliographie Critique*, Paris, G. Beauchesne, 1929, pp. 392.
- G. Gentile, *Storia della Filosofia Italiana dal Genovesi al Galluppi*, 2 vols., 2nd revised ed., Milan, Fratelli Treves, 1930, pp. xv + 272; 260, L. 40.
- E. Levinas, *La Théorie de l'Intuition dans la Phénoménologie de Husserl*, Paris, F. Alcan, 1930, pp. 223, 30 fr.
- D. Essertier, *La Sociologie (Philosophes et Savants Français du XX^e siècle, V)*, Paris, F. Alcan, 1930, pp. viii + 450, 30 fr.
- W. L. Lorimer and A. E. Taylor, *John Burnet* (Proceedings of the British Academy, Vol. XIV), London, H. Milford, pp. 28, 2s.
- J. W. Bridges, *Psychology, Normal and Abnormal: With special reference to the needs of medical students and practitioners*, London, D. Appleton & Co., 1930, pp. xxii + 552, 12s. 6d.
- K. A. Menninger, *The Human Mind*, New York, A. A. Knopf, 1930, pp. xiv + 447 + xi, 21s.
- A. Meek, *The Progress of Life*, London, E. Arnold & Co., 1930, pp. 193, 10s. 6d.
- A. H. B. Allen, *Pleasure and Instinct*, London, Kegan Paul, 1930, pp. ix + 336, 10s. 6d.

- F. Fearing, *Reflex Action: A Study in the History of Physiological Psychology* (London, Baillière, Tindall & Cox, 1930, pp. xiii + 350, 30s.
- M. de Biran, *The Influence of Habit on the Faculty of Thinking*, trans. by M. D. Boehm (Psychology Classics, Vol. III), London, Baillière, Tindall & Cox, 1929, pp. 227, 22s. 6d.
- W. Healy, A. F. Bronner, and A. M. Bowers, *The Structure and Meaning of Psychoanalysis*, New York, A. A. Knopf, 1930, pp. xx + 480 + xxiv, \$5.
- R. Money-Kyrle, *The Meaning of Sacrifice* (International Psycho-analytical Library, No. 16), London, Hogarth Press, 1930, pp. 273, 18s.
- J. Piaget, *The Child's Conception of Causality*, London, Kegan Paul, 1930, pp. viii + 309, 15s.
- Études de Psychologie pédagogique* (Archives de Philosophie, Vol. V, Cahier II), Paris, G. Beauchesne, 1927, pp. 163.
- G. Rossolimo, *L'individualité de l'enfant*, trans. from the Russian by V. Kovarsky, Paris, F. Alcan, 1929, pp. 61.
- É. Durkheim, *Le Suicide: Étude de Sociologie*, New edition, Paris, F. Alcan, 1930, pp. xii + 462, 45 fr.
- M. Halbwachs, *Les Causes du Suicide*, Paris, F. Alcan, 1930, pp. viii + 516, 70 fr.
- Baron Descamps, *Le Génie des Religions*, 2nd ed., Paris, F. Alcan, 1930, pp. xxii + 539, 30 fr.
- F. E. Williams, ed. by, *Some Social Aspects of Mental Hygiene*, Philadelphia, American Academy of Political and Social Science, 1930, pp. iv + 214, \$1.
- A. Bayet, *La Morale des Gaulois*, Paris, F. Alcan, 1930, pp. xi + 295, 35 fr.
- R. Mannier, *Mélanges de Sociologie Nord-Africaine*, Paris, F. Alcan, 1930, pp. 220, 15 fr.
- E. C. Tolman, C. H. Honzik, and E. W. Robinson, *The Effect of Degrees of Hunger upon the Order of Elimination of long and short Blinds* (University of California Publications in Psychology, Vol. IV, No. 12) Berkeley, Calif., University of California Press, 1930, pp. 189-202, \$0.25.
- R. H. Bruce, *The Effect of Removal of Reward on the Maze Performance of Rats* (University of California Publications in Psychology, Vol. IV, No. 13), Berkeley, Calif., University of California Press, 1930, pp. 203-214, \$0.25.
- Festschrift Th. G. Masaryk zum 80. Geburtstage, Part I*, Bonn, F. Cohen, 1930, pp. vii + 269.
- W. Iwanow, *Die Russische Idee*, trans. into German by J. Schor, Tübingen, J. C. B. Mohr, 1930, pp. viii + 39, M. 1.80.
- T. Lessing, *Europa und Asien (Untergang der Erde am Geist)*, 5th fully revised edition, Leipzig, F. Meiner, pp. viii + 358, M. 7.80.
- E. H. Packard, *New England Essays*, Boston, Four Seas Co., 1929, pp. 191, \$1.50.
- P. Choissard, *La Méthode Statistique et le Bon Sens en Astrologie Scientifique*, Paris, F. Alcan, 1930, pp. 114, 15 fr.
- II^e Congrès International de Graphologie* (June 1928), Paris, F. Alcan, 1930, pp. 294, 30 fr.
- É. Faguet, *Histoire de la Poésie Française, IV: J. de la Fontaine*, Paris, Boivin et Cie., 1930, pp. 357, 15 fr.
- R. B. Cattell, *Cattell Group Intelligence Scale, Specimen Set*, London, G. G. Harrap & Co., Ltd., 3s. 6d.

VII.—PHILOSOPHICAL PERIODICALS.

JOURNAL OF PHILOSOPHY. xxvi., 26. **J. Dewey.** 'The Sphere of Application of the Excluded Middle.' [Concerns the application of formal logical principles to the course of events, or 'existence,' apropos of E. Nagel's article in xxvi., 18. A logical principle, like Excluded Middle, does not "directly apply" to existences either past, present or future, and all attempts to show this beg the question. For "no number of propositions can exhaustively determine any concrete existence." Identity, contradiction and excluded middle are merely logical categories irrelevant to existence, and become not so much "false as nonsensical or meaningless when applied to things." So to declare that a door must be either open or not open overlooks (1) that "the door may be opening or shutting, that is, in process . . ."; (2) that "there is no existent door which is one hundred per cent. shut. It is shut *enough* for certain practical purposes, but it is also open—there are cracks."] **E. Nagel.** 'Can Logic be Divorced from Ontology?' [A reply to the above, which urges that logic must have ontological implications. But the writer does not seem to apprehend that Dewey's point was not that logical principles were inapplicable but that the predictions based on them were precarious and not certain *a priori*.]

xxvii. (1930), 1. **W. T. Bush.** 'Religion and Art.' [Continues comment on *Art and Civilisation* and distinguishes between the primary or æsthetic value of art and its secondary value as a vehicle of ideas. The latter must be added to make a work of art not merely 'beautiful' but also 'great.' Hence the importance of religious art, which, like Greek sculpture and Christian art until the seventeenth century, is an important social bond for holding a civilisation together.] Contains also a full and appreciative review of Dewey's *Quest for Certainty*, by Prof. C. I. Lewis. xxvii., 2. **E. W. Hall.** 'Of What Use are Whitehead's Eternal Objects?' [Whitehead's 'eternal objects' have the function of accounting for identity, permanence, universality, abstractness and potentiality, but he is tending more and more, especially in his *Process and Reality*, to transfer these functions to concrete 'occasions' *alias* 'events' *alias* 'actual entities.' So "we cannot avoid wondering whether eternal objects have not virtually become so much useless baggage." *Process and Reality* "gives at least three answers to the question, Are not eternal objects simply aspects of actual occasions, with no status of their own or by themselves?" It answers *yes*, *no*, and *yes and no*. The third of these answers occurs in the form that "God's primordial nature is the actual home of the multiplicity of eternal objects": so "God saves Whitehead from taking his own 'ontological principle' seriously." xxvii. 3. **C. M. Perry.** 'Essences Sublimated.' [In order that they may cease to "constitute a menace". Hitherto "essences have supplied a metaphysical need. In the midst of the universal flux of pragmatism, relativism, and similar doctrines, they have furnished little islands of identity where many a shipwrecked metaphysician" took refuge. But landing on these islands was always difficult. It is not enough to insist that essences are the permanents implied in change to which the changes are referred: they, too, must be "internally affected by change; they come into existence as sections of

change and as fruits of change; without change they could not be real. They are united with change in a wedlock of tragic tension." So the real is both change and permanence, flux and identity, *i.e.*, contradictory. Moreover "identity is as multiform as change," and "essences are nothing but identities taken in the widest sense conceivable". To support his "dialectal realism" the author appeals to Hegel, and declares that "relations are undeniably internal and external at the same time." Further "the world moves, not because of any '*nisus*' or pull or shove, but because it is so inherently contradictory that it cannot remain at rest".]

I. Knox. 'Tolstoy's Aesthetic Definition of Art.' ["Art is the infectious communication of emotions" and "expression for the sake of communication."] **H. A. Larrabee** reports on the Twenty-ninth Meeting of the Eastern and Western Divisions of the American Philosophical Association, at Columbia, New York, Christmas, 1929. xxvii. 4. **L. E. Akeley.** 'Methodology in Physics and Psychology with Philosophic Implications, I.' [Concludes "the traditional idea of the physicist has been that we are measuring dimensions of some physical reality utterly devoid of secondary qualities, physical objects like atoms, electrons, and the ether being only the primary qualities of matter. This view has dominated physical science, making the physicist's world utterly inconceivable to common sense." Similarly Galileo, under the influence of Platonic philosophy, made "certain postulations" which lead to absurdities.] **Y. H. Krikorian.** 'The Meaning of Purpose.' [Gives an empirical description of purpose under the four heads of expected result, subordinate acts, future reference and uncertainty, pointing out that the postulate of mechanism need not be denied to assert freedom.] xxvii., 5. **L. E. Akeley.** 'Methodology in Physics and Psychology with Philosophic Implications, II.' [Suggests that the cure for the paradox that physics reduces the world to pointer-readings (measurements) while immediate experience yields a world recalcitrant to measurement and we are yet bidden to believe that when the former impinge on the brain a transforming miracle 'emerges,' is to revise Galileo's postulate that the secondary qualities are 'subjective'. Away, therefore, with the dualism between "measurements without anything measured and realities unmeasured, but demanding measurement," and "supremely worth measuring"! "The physicist must measure his stuff with just the same kind of purpose and attitude toward it that the carpenter has when he measures his lumber," and never forget "the intimate union of the measured and the measurements . . . of the qualitative and the quantitative, of the so-called mental and the so-called material."] **J. Mackaye.** 'The Theory of Relativity: For what is it a Disguise?' [All that Einstein did was to propound new definitions of 'time' and 'length' instead of Newton's. How, then, was he able to predict the behaviour of the real? To answer this question the author assumes a 'dynamic ether' "filled with radiation of super-frequency and hence super-penetration, moving in all directions, essentially as starlight does, and having the same velocity—186,000 miles a second". 'Matter' as well as 'light' is "a modification of this field of radiation which transforms a minute fraction thereof into a form less absorbed by matter than the normal". Change of motion is due to "unbalanced radiation-pressure," and "the relativity definitions of time and space are disguises for, and mathematical equivalents of, the Doppler-displacements in the radiation". Seventeen verifications of this theory are then propounded, and the conclusion is that Einstein has discovered "the dimensional disguise for the Doppler-displacements of a

radiant ether which are inevitable if human beings . . . inhabit a dynamic, instead of a static, universe".] xxvii., 6. **S. Hook.** 'A Personal Impression of Contemporary German Philosophy.' [A brilliant study of the absurdities, and pathos, of the philosophic life as it is lived in Germany at present. Nicolai Hartmann, and especially Hans Reichenbach, come in for some praise.] xxvii., 7. **C. A. Baylis.** 'Meanings and their Exemplifications.' [Using 'meaning' rather than 'universal,' the author sets himself to account for the nature and validity of *a priori* knowledge. "A meaning is that which is connoted by any term (other than a proper name)," while "entities which are not connotable by any term (other than a proper name) are particulars," e.g., "this table and my present sense-data". We can choose our meanings and change them at will, and make sure of them, because "we can mean just what we choose to mean," and as we can define as we will "we can determine the whole nature of any meaning *a priori*". Meanings also have the relation of "logical inclusion" to other meanings, and this distinguishes them from particulars. Exemplification is not essential to meanings, which are even "independent of being meant, in the sense that certain propositions are true about them whether they are meant or not". So that "meanings are eternal and unchangeable". On the other hand "any particular is bound to exemplify, and also to fail to exemplify, an infinite number of meanings". And "unexemplified meanings are empty: particulars exemplifying no meanings are blind, i.e., meaningless"] **J. Dewey.** 'The Applicability of Logic to Existence.' [A reply to E. Nagel in xxvi., 26, which admits that "there was considerable ambiguity of statement" in his previous article.] Contains also a long review of Eddington by E. B. McGilvary.

REVUE DE MÉTAPHYSIQUE ET DE MORALE. 36^e Année, No. 2, Avril-Juin, 1929. **L. Weber.** *De quelques caractères de la pensée symbolique.* [The first instalment of an article in which the author sets himself to distinguish between two kinds of symbolic thinking (i.e. thinking by means of symbols), viz., using mathematical symbols and using ordinary language. Starting from Leibniz's well-known definition of symbolic thinking, in his *Méditationes de Cognitione, Veritate et Ideis*, he accuses Leibniz of having failed to observe the profound difference between these two kinds of symbols and their uses. To say that "mathematical" thinking is precise and "discursive" thinking vague is true, but does not go to the root of the matter. For, whilst all thinking abstracts, the real difference lies in the manner of abstracting. Again, whilst all thinking by means of symbols has an automatic, mechanical character, there is a great difference in the respective automatisms. Mathematical thinking is an operation with symbols for terms and relations which maintain, throughout the operation, their strict identity. In mathematical abstraction the more general a concept is the richer is its content. In verbal or discursive thinking abstraction is effected by resemblance and analogy. Hence, meanings are ever fluid and shifting, eked out by imagery, suffused by emotion, not strictly controlled by the law of contradiction. All "non-mathematical" thinking is an attempt to "express the inexpressible". Words are unavoidable, yet they ever play us false.] **G. Urbain.** *Remarques sur l'orientation des doctrines chimiques.* [A survey of present-day tendencies in chemical theory in the light of their historical context.] **Charles Renouvier et William James.** *Correspondance, éditée par R. B. Perry.* [The concluding instalment of this correspondence, covering the years 1883 to 1896. Most interesting, and philosophically important,

are the letters of 1887-88 in which Renouvier and James discuss Josiah Royce's *The Religious Aspect of Philosophy*, and especially the argument that the existence of error implies the existence of a single, all-inclusive Mind. James declares the argument to be "unanswerable in my eyes," though expressing misgivings about its monistic implications.] **M. Winter.** *La Physique indéterministe*. [A brief discussion with special reference to the work of Heisenberg and Born, of recent tendencies in physical thought concerning the theory of quanta and criticisms of it.] **R. Lenoir.** *Anthropologie et Sociologie*. [A learned and allusive essay, of which it is very difficult to say what is its aim or its outcome. Incidental epigrams impress, e.g., "*Quiconque étudie les sociétés a le devoir de n'en jamais sourire*". Apparently, the author's main point, made by means of a discussion of magic and myth, is to remind scientifically-minded anthropologists how difficult it is to understand the inwardness of minds dominated by magic and expressing themselves through ritual, and how wrong it is to despise or look down upon primitive minds, under the influence of Comte's "wholly imaginary" law of three stages of evolution from fetishism to science.] *Études Critiques*. **V. Jankelevitch.** *Bergsonisme et Biologie*. [An appreciative review, by a whole-hearted Bergsonian, of a book by Von Monakow and R. Mourgue, entitled *Introduction biologique à l'étude de la neurologie et de la psychopathologie, intégration et désintégration de fonction*. The author claims that this book, in definite opposition to the Cartesian fashion of "mechanistic" thinking in biology, is a return to the "true French tradition which is vitalistic". Its main value lies in its sharp distinction between a function and its localisation, i.e., the cerebral mechanisms which the function at once employs and transcends: *le mécanique est moins étendu que le fonctionnel*. Other important concepts are that of a general *hormé* (cf. Bergson's *élan*) behind all special functions, and of the *positive* character of disease.] *Questions Pratiques*. **G. Morin.** *L'abus du Droit et les relations du Réel et des Concepts dans le domaine juridique*. [The doctrine that rights can be "abused" has, ever since 1855, been used by French courts with increasing frequency in a great variety of cases, and in the end with results clearly in conflict with the individualistic philosophy underlying the Revolution doctrine of the "Rights of Man" and the *Code Civil*. The author criticises the attempts made by Josseland and other writers on jurisprudence to reconcile the new practice with the old theory; also Duguit's attempt to provide a satisfactory theoretical basis for the new practice by means of his concept of social function. He thinks it necessary to recognise openly that modern societies require a new philosophy of law, according to which law exists, not to formulate and protect "rights" inherent in the individual as such, but to define and assure a social order giving to all its members a guarantee of at least a minimum of opportunity for making the most of themselves, physically, intellectually, morally. The old philosophy of law was, in fact, if not in intention, a respecter of property and, therefore, of *Things*; the philosophy of law of the future will put human values first: it will be a respecter of *persons*.] Reviews of books, French and Foreign. Abstracts of Periodicals.

36^e Année, No. 3, Juillet-Septembre, 1929. **R. Berthelot.** *Lamarck et Goethe: L'évolutionnisme de la continuité au début du xix^e siècle*. [By "evolutionism of continuity" the author means the theory of continuous metamorphosis of living beings which, based on the one hand on observation of the growth from embryo to adult, on the other on comparative anatomy, led both Goethe and Lamarck to challenge the theory of dis-

continuous, fixed species, which found its classical embodiment in Linné's system of classification. The author traces the various nuances of the continuity theory in the thought of biologists and philosophers who influenced Goethe and Lamarck. He points out the ambiguity in Goethe's handling of the theory, by which it is left obscure whether the continuity is conceived as an evolution of living forms in time, or merely as a continuous affiliation of forms defeating all attempts at rigid classification.]

L. Weber. *De quelques caractères de la pensée symbolique.* [Completes the article begun in the preceding issue. After briefly summarising the distinction made in that article between "operative" or "mathematical" and "nominal" (= verbal) or "discursive" thinking, the author reveals the real purpose of his whole argument, which is to criticise theological language and thought as akin, in its use of symbols, to dreaming. He does this by an elaborate critical analysis of passages from Bossuet's writings about divine creation, eternity, etc. He finds the "latent" wish behind these theological dreamings in a desire to conceive the universe as wholly intelligible, in the sense of deducible from, or explicable by, a single ultimate principle. Of course, the concluding suggestion is that the satisfaction achieved is illusory.] **F. de Sarlo.** *L'activité représentative et l'imagination.* [The author distinguishes "representative activity," by which he means all awareness of "contents," from "cognitive" activity (judgment) and "practical" activity (valuation and action). The latter two modes of activity presuppose the former, but the former is distinct and has laws and, so to speak, a life of its own. This life is to be seen in the free constructions of fancy, in play, and in art. The author's main points are (i) that, in all these three forms, representative activity has many different levels or planes, and (2) that at all levels it may be practised for its own sake, and for its own inherent worthwhileness or satisfaction. Hence, he combats all purely hedonistic or utilitarian theories of play, e.g., though play may for the young be incidentally a preparation for life, it is for both young and old essentially an exercise of the imagination indulged in for its own sake.]

Études Critiques. **B. Lavergne.** *Doctrines sociales et science économique.* [A review of G. Pirou's book of the same title. The reviewer is chiefly concerned to point out the rapprochement, in present-day economic theory, of the adherents of the "deductive" and "inductive" methods, and, in general, of different schools of economic thought, foreshadowing a coming synthesis of economic doctrines.] *Questions Pratiques.* **G. Gurvitch.** *Le principe démocratique et la démocratie future.* [A vigorous defence of democracy against its critics. Democracy is, in essence, a synthesis of three great ideals, viz., (i) sovereignty of the people; (ii) equality; and (iii) individual liberty. These ideals must be interpreted in close relation to each other, within an organic theory of the state which admits no fixed hierarchy of status, but treats every individual as an organ of the social whole with a distinctive function. So conceived, democracy is not necessarily individualistic or atomistic: on the contrary, it alone holds the balance between one-sided individualism and equally one-sided glorification of the state. Equality is meaningless, except as equality before the law based on the individual's function in the whole. The future of democracy is assured, for it is spreading from the purely political into the economic field (democratic organisation of industry) and into the international field (League of Nations). In ultimate analysis, democracy is identical with socialism—with the organisation of society on the principle of "the sovereignty of social right."] Reviews of new books, French and Foreign. Periodicals.

36^e Année, No. 4, Octobre-Decembre, 1929. **L. de Broglie.** *Déterminisme et Causalité dans la physique contemporaine.* [Points out that recent developments in physical theory tend to destroy the classical conception of every event as rigidly determined in its time and place, so that its occurrence might be calculated with absolute certainty by an omniscient mind such as Laplace imagined. Instead, the differential equations of modern Physics determine, not future events themselves, but merely the probability of their occurrence. "They are not causal laws: they are laws of probability."] **V. Delbos.** *La préparation de la Philosophie moderne: Caractères généraux de la Philosophie moderne.* [A long first instalment, published posthumously, of a course of lectures given by the author at the Sorbonne. Some sections of the text are merely brief notes and headings which must have been verbally elaborated in the lecture-room. Other sections are fully written out and seem to have been delivered *verbatim*. In this instalment Delbos emphasises, first, the "return to antiquity," and secondly, "speculation about Nature," as determining the distinctive character of modern philosophy. Extensive bibliographical references are scattered through the text.] **J. Wilbois.** *La notion philosophique de cause dans le Monde social.* [In Sociology we must distinguish four kinds of causes in seeking the explanation of any given event, *viz.*, (1) material conditions; (2) psychological factors; (3) social forms; (4) *faits déclenchants*, *i.e.*, accidental happenings which deflect, or set off, the other causes. The author gives a number of concrete illustrations to show how all these four causes are necessary for the complete analysis of the occurrence of any social phenomenon, be it a case of suicide, or the rise of Bolshevism in Russia. In the second half of his article he traces at length the interactions and linkages (*liens*) between the four kinds of causes, and concludes with a programmatic statement about the nature of Sociology as a "science," *viz.*, (a) a science of laws; (b) a science of types; (c) a science of evolution. The main point is that Sociology must emancipate itself from the fashion of trying to understand social facts by means of concepts drawn from the mechanical and physical sciences.] **J. BARUZL.** *La psychologie de l'Art, par Henri Delacroix.* [An appreciative critical review.] Reviews of books, French and Foreign. Periodicals. Announcement of the Seventh International Congress of Philosophy.

KANT STUDIEN. Band xxxiv., Heft 3-4, 1929. Pp. 275 to 572. *Erich Becher*, by **Paul Luchtenberg**. [Obituary notice, with portrait and bibliography.] *Friedrich Kuntze*, by **Hans Sveistrup**. [Obituary notice, with portrait.] *Philosophie und Schicksal*, by **Paul Tillich**. [An inaugural lecture, unintelligible to the reviewer. Purports to deal with the two questions, "What destiny has led Philosophy to regard itself as destined?" and "How can Philosophy conceptually apprehend its subjection to destiny?"] *Samuel Grubbe*, by **John Cullberg**. [Account of the work of a Swedish exponent of transcendental idealism (1786-1853). Useful as a supplement to the short account given in Ueberweg's *Grundriss*, part v., pp. 245 f. (12th ed.).] *Die Schule Karl Barths und die Marburger Philosophie*, by **H. W. van der Vaart Smit**. [Study of the philosophical content of Barth's theology. Finds that his theology rests on revelation, using Kantian technique merely to develop dogmatic principles; and that it takes Kant's first Critique as leading to and warranting the positive attitude towards moral and religious values expressed in the two later critiques. The Marburg school, on the contrary, is strictly rationalistic, and regards the two later critiques as retrogressive and worthless.] *Zur*

Metaphysik der Seele, by **S. Frank**. [A dignified plea for the setting alongside the psychological account of mental phenomena a metaphysical account, won from within, of the soul from which the phenomena spring. "It is altogether remarkable that mankind is able to live through certain relatively long periods without cherishing a scientific interest in itself, without arriving at a conception of the meaning and inner nature of its own life."] *Zum Problem einer theoretischen Biologie*, by **L. von Bertalanffy**. [Desiderates a theoretical biology analogous to theoretical physics, i.e., a deductively fruitful but strictly scientific body of laws. Both mechanistic and vitalistic biology are metaphysical. Biology cannot dispense even in description with the historical and the teleological in vital phenomena. The problem is how to retain them in a scientific way. An exemplary article.] *Moses Mendelssohn und Immanuel Kant*, by **W. Kinkel**. [Occasioned by the bicentenary of the former's birth, Sept. 6, 1929.] Reviews, communications and reports. Supplementary brochure, *Metaphysik oder verstehende Sinn-Wissenschaft? Gedanken zur Neugründung der Philosophie im Hinblick auf Heidegger's "Sein und Zeit."*

ANNALEN DER PHILOSOPHIE. Band viii., Heft 7 and 8. Nov. 1929. **J. E. Heyde**. *Entwicklung als Auswicklung?* [Evolution is distinguished from mere change by the addition of something new. The theory that what has evolved pre-existed in some form will not bear examination. The correct expression is not that B evolved out of A but that A evolved into B. The frank recognition of the fact that something is added brings the concept of evolution back to that of creation, which it was intended to exclude.] **A. Herzberg**. *Das Stabilitätsprinzip in der modernen Psychologie*. [The tendency to stability is the principle of all activity, as well mental as physiological and physico-chemical.] **Č. Fries**. *Der Zufall*. [Obscure defence of accident or chance in terms of the writer's theory of an Urville, not here expounded.] **M. T. Selesković**. *Das Wunder*. [Aphoristic outline of an agnostic mystical metaphysic; replaces the claim to an absolute (non-relativistic) knowledge of reality by the attitude of wonder.] **H. Bauermeister**. "Leben" und "Geist." [Adverse note on Klages' restriction of the term "life" to impulse and on his placing it in complete opposition to "spirit." Spirit limits impulse in the service of life.] Reviews.

Band viii., Heft 9 u. 10. April, 1930. **W. Dubislav**. *Joseph Petzoldt in memoriam*. [Bibliography appended.] **H. Triepe**. *Körper und Vorgänge*. [Physical reality being analysable into bodies and events, perhaps ultimately into events, space and time should appear in metaphysics only as abstractions from, relations of, the former.] **P. Krückmann**, *Fiktionen im Recht*. [Criticism of certain details of Egbert Munzer's "Ueber Gesetzesfiktionen".] **E. Barthel**. *Erweiterung raum-theoretischer Denkmöglichkeiten durch die Riemannsche Geometrie*. [By Riemannian geometry the author means the geometry of a finite space. In a long article he develops its consequences for astronomy.] **E. Ruckhaber**. *Logische und sprachliche Verneinung*. [Verbal negation is conventional, logical negation is absolute. In the light of this distinction, plus and minus signs in mathematics are clearly conventional. So-called negative numbers have therefore no intrinsic peculiarity.] Reviews. Index, etc., to vol. viii. The next volume will appear under new editorship and with the new title *Erkenntnis*. The reason is the withdrawal of the editor hitherto, Dr. Raymund Schmidt, for the founding of a new periodical.

VIII.—NOTES.

LOGIC FOR USE.

IN Prof. Leonard Russell's review of Dr. Schiller's latest book (*MIND*, April, 1930) two points call for some notice: the suggestion that the Formal Logic attacked by Dr. Schiller exists only in his imagination; and the suggestion that Formal Logic when developed into Symbolic Logic has a function of real importance in the discovery of truth.

Prof. Russell readily grants (219) that "the old traditional formal logic was too restricted in its scope, and not altogether satisfactory in the assumptions on which it rested"—a kind of faint damnation which almost amounts to praise. No doubt even the hardest-shelled among modern formal logicians would admit some possible room for enlargement of the system, and some lack of complete satisfactoriness in its foundations. But Dr. Schiller has already often brought forward more definite objections than these agreeable ones. It is rather the unwarranted extensions of its scope than its petty restrictions that he condemns. In his book on Formal Logic he makes a detailed attack which no formal logician could comfortably accept. Again, in the present book, there are many passages offering the same kind of definite challenge. These accusations are not met by vaguely admitting room for improvement.

When some of the petty restrictions are removed, even the existing Formal Logic might conceivably be *used* in such a way as to deprive it of its harmful qualities; and such special use of it is perhaps what Prof. Russell, hoping for the best, is content to see in the system. But Schiller is interested, rather, in the difficulty that is actually found in keeping Formal Logic sufficiently restricted to this use and so avoiding the error of *verbalism*. That is the evil that he always has in view when he attacks Formal Logic, and the question how far this common weakness of thought is encouraged by Formal Logic matters less than the question how the temptation itself may best be resisted. Indeed with the latter question in view Formal Logic, at its worst, would appear as verbalism barefaced, and on that account perhaps less insidious than when it is decently veiled. Even if we condemn Formal Logic for pretending to show a solid foundation for verbalism, it is the subtler forms of that human failing that naturally do the most harm.

There are many signs by which these subtler forms can be detected. Among them are: (1) the assumption that every 'proposition' (i.e., every professedly assertive *sentence*) carries its meaning clearly on its face for all who know the generally accepted meaning of its words; and (2) the assumption that every concisely expressed 'proposition' is an indivisible unit, and therefore must be either true or false.

It is possible that the verbalists, if asked whether they make these assumptions, would disclaim them; and even probable that in fact they do not make them quite universally. Here and there, no doubt, they—like every one else—can recognise a difference between sentence and assertion, and between a 'single' concise assertion and a complex one. We need not suppose that it is only the perfectly consistent verbalists—if any there be—who are called to account by Dr. Schiller. Rather it is our

frequent lapses into verbalism that he deplors. And there is plenty of evidence in all his books that he finds these lapses frequent even among philosophers of some repute. For example, the philosophical doctrine that "Ultimate Reality is such that it does not contradict itself" is typical of the confusion between 'diction' and the realities it tries to express. Ultimate Reality does not make verbal statements about its own nature, and *therefore* is safe against using any kind of defective diction. This example is briefly referred to in a note at page 133 of the book. Some others, equally respectable, are noticed at pages 11, 12, 17, 65 and 237.

Prof. Russell's second suggestion is more novel and perhaps more important. It has also the merit of being the first relevant objection to pragmatism (i.e., anti-verbalism) that has yet been raised in all the years since William James first adopted Peirce's view. The many efforts hitherto made to show reasons for not admitting this subversive method into philosophy have had one feature in common: that they attack not what the pragmatists have explained to be their meaning, but what the verbalists have too eagerly supposed them to mean.

We might even regard Prof. Russell's objection as friendly and helpful rather than controversial. It is true enough that pragmatism, in its present forms, has not tried to assimilate Symbolic Logic, and possibly it might do so with advantage. One difficulty is that Symbolic Logic shares with Formal Logic the assumption that the risk of ambiguity may be safely neglected, but there is nothing to prevent our conceiving the occasions suitable for such neglect in a more restricted way than Formal Logic has ever expressly contemplated. Instead of imagining that Symbolic Logic can serve the purpose of directly giving us 'proved' conclusions about matters of fact, we might use it, as Prof. Russell seems to propose, in preparing the ground for experimental work. What is more difficult at first to see is how we could then distinguish it from mathematics—or why we should try to do so. The symbols used would presumably be different, but the method would be in essence the same, namely that of neglecting the risk of ambiguity for certain limited purposes. Just in so far as the purposes are intentionally and consistently limited Symbolic Logic would escape the charge of verbalism. Verbalism (the pragmatists would say) consists partly in the attempt to make words do a kind of work for which they are not fitted. Specially manufactured symbols can more easily confess the artificial character of their edges.

Anyhow, Dr. Schiller might well be excused for leaving to others, and presumably to the mathematicians, the enterprise of improving logic in this new direction. The main purpose of his book was to improve what is now generally known as logic (whether confessedly formal or not) by shedding the verbalism that still clings to it. This remains a pressing and difficult task in spite of all that has in recent years been done towards it. For obvious reasons the trouble presses more in philosophy than in science.

ALFRED SIDGWICK.

BIOLOGICAL PRINCIPLES.

TO THE EDITOR OF "MIND".

SIR,

Permit me to try to remove some misunderstandings contained in the review of my book, *Biological Principles*, in the last number of *MIND*.

My excuse for wishing to do so is that the issues involved are not confined to this book but concern the whole question of the relation between the natural and the philosophical sciences. There is an enormous hiatus between particular empirical natural sciences and those general *a priori* philosophical sciences which are ultimately involved in the systematisation of the data furnished by the former. Only in the case of pure mathematics—in so far as it has developed in close relation to the needs of physics—is this hiatus filled up. Now followers of branches of natural science other than physics, observing the brilliant success of the latter, and being too much occupied with their own problems to study the more general logical principles involved, are apt to conclude that all they need to do is to follow closely the example of physics and their own sciences will automatically emerge to perfection. But if one takes a more detached point of view it is not difficult to see that this supposition may, in the long run, prove to be mistaken. From the standpoint of modern studies in mathematical logic it would appear to constitute an undue restriction of what logic may ultimately have to offer to the empirical sciences. This is one point which I have tried to urge upon the consideration of biologists in the above book. But, unfortunately, the situation is not so simple as this. Epistemological and ontological questions of great difficulty are also involved. My starting point is the well-known antithetical character of biological thought. It is not difficult to see how prejudicial the existence of these antitheses, and the resulting controversies, has been to the free development of biology, and a little consideration of them suffices to show that they spring very largely from the presuppositions which biologists *bring to* the study of their subject matter, not merely from the difficulties of that subject matter itself. Consequently these obstacles to biological development require investigation by the "philosophical method" as well as by the "scientific". The above book is concerned with them *only* from the philosophical point of view as the "logical clarification of thoughts" (Wittgenstein) and the "study of the *a priori*" (C. I. Lewis). I tried to make this clear in a Preface and a somewhat lengthy Introduction (to neither of which the reviewer refers), but it is one of the most difficult things in the world to explain to a man of science that there is such a thing as a "philosophical method" and that (in the words of C. I. Lewis) there are "such problems of fundamental concepts and classification" which science "of itself cannot solve because, in the nature of the case, they are antecedent to the investigation". It seems to be for this reason that the reviewer has missed the wood for the trees, and has concentrated on my remarks on empirical matters, remarks to which, as I expressly state in the Preface, I do not wish to attach much importance. The serious blemishes in the book are philosophical ones, arising out of my own philosophical incompetence, but the reviewer has not "spotted" any of these. Yet it seems to be the duty of a reviewer of such a book in a *philosophical* journal to point out philosophical blemishes, in the hope that philosophers who are interested in science may be persuaded to help towards their removal. But it is hardly to be expected that any philosopher, after reading this review, will suppose that the book has any concern with philosophy at all. All this illustrates, in a depressing way, the difficulties encountered by any attempt at a *rapprochement* between the philosophical and the natural sciences. Exponents of the latter seem to regard philosophy as *either* an inferior rival and alternative institution to their own, *or* as a sort of constable who keeps the peace between science and religion by means of a cudgel, in the shape of philosophical scepticism, which is flourished from

time to time when science gets into difficulties. In the above book I have essayed the seemingly thankless task of defending scientific knowledge against some of these assaults. I am tempted to agree with A. N. Whitehead in "assuming as an axiom" that "science is not a fairy tale" and that it is not "engaged in decking out unknowable entities with arbitrary and fantastic properties". But, as far as I can gather from his various works, Dr. Needham seems to take the opposite view, and this is perhaps one reason why we fail to understand one another.

In view of these major divergences it is hardly necessary to refer to minor misunderstandings. But I should like to point out that although Dr. Needham seems to make me assert that "*characters* such as pink *x* and curly *y* may well be inherited but not *parts* such as liver and gonad," no such assertion is to be found in the book. On the contrary I devote seven pages to explaining that propositions of the form "*x* is inherited" (whether "*x*" is a part or a character) are senseless, a fact which is now understood by most geneticists. Also, although I can hardly be expected to have known that "the Professors of Biochemistry at both Oxford and Cambridge were lecturing on the very subject of organisation in the living cell" at the time my book was being printed, I *did* quote one of those professors as urging the importance of organisation. The fact that its importance is understood by a few leading authorities does not alter the fact that this is not recognised by the majority, and that the veteran cytologist, E. B. Wilson, has recently said that "we are unable to define precisely the meaning of this vague term". I have said no "unpleasant things" about *biochemists* or their wonderful achievements in biochemistry, but I have drawn attention to some extravagant and demonstrably false assertions which they have made about *biology*, and I deplore the contemptuous attitude they are apt to adopt towards it. If we are to take organisation seriously it is important that we should try to discover the precise range of application of the adjective "chemical" and of chemical notions generally. Biochemists might help in this, instead of supposing that they have an unlimited range. Notions of unlimited range are generally useless, and it is clear that chemical ones cannot be applied to bath-chairs and electrons: their range is somewhere between such terms as these in the scale of magnitudes.

J. H. WOODGER.

SEVENTH INTERNATIONAL CONGRESS OF PHILOSOPHY.

The programme of the Congress will be as follows:—

A.—GENERAL PLAN.

MONDAY EVENING, 1st SEPTEMBER—

Opening Meeting. Address of welcome, and reply by Representatives of Foreign Delegates.

TUESDAY, WEDNESDAY, THURSDAY AND FRIDAY, 2ND TO 5TH SEPTEMBER, MORNINGS, 9.30—12.30.

Simultaneous Section Meetings in all Four Divisions.

TUESDAY AFTERNOON.

General Session. Division A.

WEDNESDAY AFTERNOON.

General Session. Division B.

THURSDAY AFTERNOON.

General Session. Division C.

THURSDAY EVENING.

Business meeting.

FRIDAY AFTERNOON.

General Session. Division D.

B.—SESSIONS.

DIVISION A.—METAPHYSICS.

GENERAL SESSION.—Are recent advances in Physics of metaphysical importance?

Chairman.—Prof. J. A. Smith.

Prof. A. Liebert, Prof. A. Aliotta, Prof. F. Enriques, Prof. Jorgen Jorgensen.

SECTION 1.—Is a philosophy of history consistent with the facts of history?

Chairman.—Prof. J. B. Baillie.

Prof. Jacques Chevalier, Prof. Morris R. Cohen, Prof. N. Hartmann, Prof. R. F. A. Hoernlé.

SECTION 2.—Must biological processes be either purposive or mechanistic?

Chairman.—Prof. J. S. Haldane.

Prof. H. Wildon Carr, Prof. E. A. Singer, Prof. Emil Ungerer, Prof. J. E. Woodbridge.

SECTION 3.—The relations between Metaphysics and Religion.

Chairman.—Prof. Clement C. S. Webb.

Senatore Benedetto Croce, Prof. Edgar S. Brightman, Prof. Ph. Kohnstamm, Prof. F. de Sarlo.

SECTION 4.—Open Session.

Chairman.—Prof. L. J. Russell.

Prof. E. M. Radl, Prof. M. Schlick, Miss L. S. Stebbing, Prof. G. F. Stout.

DIVISION B.—LOGIC AND EPISTEMOLOGY.

GENERAL SESSION.

Chairman.—Prof. H. H. Joachim.

(a) The value of recent contributions to Logic.

Prof. A. Lalande, Prof. D. Michaltshev.

(b) Phenomenology.

Prof. Hans Driesch, Prof. S. M. Dasgupta.

SECTION 1.—The nature of perception and its objects.

Chairman.—Prof. H. A. Prichard.

Prof. G. Dawes Hicks, Prof. Charles Hartshorn, Mr. C. E. M. Joad.

SECTION 2.—The nature and source of non-perceptual factors in thinking.

Chairman.—Prof. G. E. Moore.

Prof. Wm. Pepperell Montague, Prof. L. Noel, Prof. H. G. Stokes, Prof. Reginald Jackson.

SECTION 3.—The relation of scientific thinking to the ideal of knowledge.

Chairman.—Prof. T. Percy Nunn.

Prof. B. Bauch, Prof. L. Brunschvicg, Prof. E. Dupréel, Prof. Raffaello Piccoli.

SECTION 4.—Open Session.

Chairman.—Mr. H. W. B. Joseph.

Prof. Malte Jacobsson, Prof. N. D. Lossky, Prof. G. D. Scraba, Prof. R. W. Sellars, Prof. A. Zoltowski.

DIVISION C.—ETHICS, POLITICS, AND ÆSTHETICS.

GENERAL SESSION.—The value of Ethical and Political Philosophy as guides in practice.

Chairman.—Prof. W. R. Sorley.

Prof. C. Bouglé, Prof. G. C. Field, Prof. W. Lutoslawski, Prof. Leo Polak.

SECTION 1.—Is the distinction between moral rightness and wrongness ultimate ?

Chairman.—Mr. W. D. Ross.

Prof. J. Laird, Prof. A. Meiklejohn, Prof. F. Medicus, Prof. D. Parodi, Dr. F. C. S. Schiller.

SECTION 2.—Is the ground of political obligation always one and the same ?

Chairman.—Prof. Norman Kemp Smith.

Prof. W. Macmahon Ball, Prof. G. Davy, Prof. Ch. W. Hendel Jr., Dr. J. Nissen.

SECTION 3.—Recent suggestions in the theory of Fine Art.

Chairman.—Mr. Justice Meredith.

Prof. S. Alexander, Prof. Richard Müller-Freienfels, Prof. Victor Kuhr.

SECTION 4.—Open Session.

Chairman.—Principal G. Galloway.

Prof. L. Kruse, Prof. Donald S. Mackay, Prof. R. A. Tsanoff.

DIVISION D.—HISTORY OF PHILOSOPHY.

GENERAL SESSION.—In what respects has philosophy progressed ?

Chairman.—Prof. J. H. Muirhead.

Prof. Emile Bréhier, Prof. Adolf Phalen.

SECTION 1.—(a) *Ancient Philosophy.*—What is alive and what is dead in the philosophy of Classical Antiquity ?

Prof. J. Stenzel.

(b) *Mediæval Philosophy.**Chairman.*—Prof. W. G. de Burgh.

The philosophical problems at issue in the thirteenth and fourteenth centuries.

Prof. Guido de Ruggiero.

(c) *Oriental Philosophy.*

What contributions have been made to philosophy by Eastern philosophers (including Jews and Arabs) ?

Prof. A. A. Roback.

SECTION 2.—Philosophy of the seventeenth and eighteenth centuries.

Chairman.—Prof. Bowman.

Has Kant by the introduction of his transcendental method rendered unnecessary the study of his predecessors ?

Prof. F. H. Anderson, Dr. A. C. Ewing.

SECTION 3.—Philosophy of the nineteenth and twentieth centuries.

Chairman.—Prof. H. Wildon Carr.

Prof. Heimsoeth, Prof. R. B. Perry, Prof. J. L. Stocks.

SECTION 4.—Open Session.

Prof. L. Robin, Prof. Ernst Cassirer.

COURSE ON THEOLOGY AND THE PSYCHOLOGY OF RELIGION.

Under the patronage of the Autonomous Faculty of Theology, in the Summer School of the University of Geneva (Switzerland), a week's course on *Theology and the Psychology of Religion* will be given from Monday, 25th, till Saturday, 30th August. Lectures will be delivered by prominent scholars from various countries, amongst others, by Prof. Maurice Goguel and Raoul Allier from Paris, Henri Clavier from Montpellier, Theodor Siegfried from Marburg, Z. F. Willis, secretary of the international Y.M.C.A. Committee, together with a number of Swiss scholars.

The subjects treated are of wide interest dealing with questions of method, psychotherapy, children's psychology, Old Testament study, Faith, Conversion, etc.

The fee for the course is 30 fr. ; for students and clergymen, 20 fr. All enquiries should be addressed to Prof. G. Berguer, Genthod, near Geneva.

INTERNATIONAL MORAL EDUCATION CONGRESS.

Educationists from all parts of the world will meet at the Sorbonne, Paris, 23 to 28 September, 1930, in a Fifth International Congress of Moral Education ; previous Congresses having been held in London, 1908, The Hague, 1912, Geneva, 1922, and Rome, 1926. The basis is the widest possible. The Congress has equally appealed, and still equally appeals, to theologians and Rationalists, Catholics and Protestants, Jews, Muslims, Hindus, Confucians, Buddhists, Shintoists. The themes to be discussed are : (1) History-teaching, (2) Discipline and Autonomy in Moral Education, and (3) Varieties of Method in Moral Education. Sir Percy Nunn is Chairman of the International Executive Council ; and for English-speaking countries a special committee organises (Hon. Sec. Mr. F. J. Gould, Armorel, Woodfield Avenue, Ealing, London, W.5).

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